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LECTURES
ON
VETERINARY MEDICINE,
DELIVERED IN THE
UNIVERSITY OF LONDON,
BY
MR. YOUATT.

LECTURE XXIII.

THE SEROUS MEMBRANES.—THE PLEURA.

THE parietes of the thorax are lined, and the lungs are covered, by a smooth glistening membrane, *the pleura*. It is a *serous* membrane, so called from the nature of its exhalation,—*serous*— albumenous — in distinction from the *mucous secretion* yielded by the membrane of the air-passages that have hitherto come under our consideration,—the serous membrane generally invests the most important organs, and always those that are essentially connected with life, while the mucous membrane lines the interior of the greater part of them. The pleura is the investing membrane of the lungs, and a mucous membrane the lining one of the bronchial tubes.

The Polish of the free Surface.—Among the principal circumstances to be observed with regard to the pleura is the polish of its external, or, as it may perhaps be better termed, *free* surface. The glistening appearance of the lungs, and of the inside of the costal wall, is to be attributed entirely to the membrane by which they are covered; for if we strip off the pleura, the surface of the lungs and of the intercostal muscles is not a little rough. This is the character by which the serous membrane is distinguished every-where, and thus the motion of the various organs is freer and less dangerous. Although the lungs and the thorax are in constant approximation with each other, both in expiration and inspiration, yet in the frequently hurried and violent motion of the animal, and in fact in every act of expiration and inspiration, of dilatation and contraction, much and injurious friction would ensue if the surfaces did not glide freely over each other, from the peculiar polish of this membrane.

We shall have to observe, by-and-by, how admirably the serous membranes prevent injurious friction, even in disease as well as in health. In health the polished surfaces glide smoothly and harmlessly over each other, and friction is prevented by easiness of motion. In disease they adhere to each other, and friction is destroyed by the impossibility of motion. Inflammation of a serous membrane is quickly followed by adhesion, for in consequence of the increased sensibility of the membrane, and in spite of the polished surface, and the lubricating fluid, pain and aggravated disease would result from the attrition of the parts. This does not so often take place in the quadruped as in the human being; for the lungs here resting on the sternum, instead of the elastic diaphragm, the motion is not so extensive; but we occasionally see in all our patients the lungs bound to the intercostals, or the opposed reflexions of the pleura not only by interposed bands, but their whole surfaces agglutinated together by firm organised adhesions.

The Exhaled Fluid.—Every serous membrane has innumerable exhalent vessels upon its surface, from which a considerable quantity of fluid is poured out. This fluid I have said is of a serous character; it resembles the serum of the blood. In life and during health it perhaps exists in the chest, only as a kind of dew, just sufficient to lubricate the surfaces. When we open the chest soon after death, we recognise it in the steam which arises, and in the few drops of fluid which, being condensed, are found at the lowest part of the chest.

The Absorbents.—The quantity, however, which is exhaled from all the serous membranes, must be very great; it is perhaps equal to, or superior to that which is yielded by the cutaneous vessels. If so little is found in ordinary cases, it is because the absorbents are as numerous and as active as the exhalents, and, during health, that which is poured out by the one is taken up by the other; but in circumstances

of disease, either when the exhalents are stimulated to undue action, or the power of the absorbents is diminished, the fluid rapidly and greatly accumulates. Thus we have hydrothorax or dropsy of the chest, as one of the sequelæ of thoracic inflammation; and the same disturbed balance of action will produce similar effusion in other cavities.

Nature of Occasional Adhesions.—The character of the fluid varies under circumstances of disease. Although albumenous—disposed to coagulate at certain temperatures, and under certain influences—it is easily and completely absorbed during health. Even in disease it sometimes retains its perfect fluidity; but at other times, whether from actual increase of temperature produced by inflammation, or from a degree of vitality or tendency to organisation communicated to it, it does coagulate over the whole of the surface of the pleura, or on spots of it, forming extensive layers or mere bands, into which vessels speedily shoot, and the process of organisation is accomplished.

Extensibility.—This property of membrane generally is no-where more strikingly displayed than in the serous membranes, and particularly in that under consideration. How different the bulk of the lungs before the act of inspiration has commenced, and after it has been completed, and especially in the laborious respiration of disease or rapid exertion! In either state of the lungs the pleura seems to be perfectly fitted to that which it envelops.

Elasticity.—This property is connected with the preceding, and as clearly demonstrated. The power which distends it being withdrawn, it contracts with the diminished bulk of the lungs. It is one agent, and perhaps of no mean power in the process of expiration. It is one illustration of that principle of elasticity to which, in the last lecture, I attributed the whole function of expiration. Accident occasionally presents us with a yet more evident proof of the elasticity, or contractility of the investing membrane. If the parietes of the thorax are punctured, and the pressure of the atmosphere equalised within and without the pulmonary pleura, the lung collapses to less than half its size, even after the act of expiration had been completed.

Vascularity.—In a state of health the blood-vessels of the pleura are not numerous. Considerable portions of the reflected part of it may be examined, without any arteries or veins appearing, and where the blood will not follow the knife. Even the vessels that are apparent seem to be ramifying over it, rather than entering into it, or belonging to it. A membrane, the expansion of which is so variable, and indeed every moment changing, does not seem to be calculated for the transmission of numerous vessels. The exhalents, however, carrying the pellucid serous fluid of which I have been speaking, communicate with, or proceed from arterial ramifications; and under inflammation there are vessels enough not previously carrying red blood which expand, and mark by their redness the seat and extent of inflammation. This is peculiarly evident in that patchy species of inflammation of the pleura sometimes accompanying rabies.

Little Sensibility.—The pleura, like other serous membranes, is possessed of very little sensibility. Few nerves from the sensitive column of the spinal chord reach it. Acute feeling would render these membranes generally, and this membrane in particular, unfit for the function they have to discharge. It has too much motion, even during sleep, and far too forcible friction with the parietes of the thorax in morbid or hurried respiration, to render it convenient or useful for it to possess much sensation. Some of those anatomists, whose experiments on the living animal do no credit to their humanity, have given us most singular proof of the insensibility, not only of these serous membranes, but of the organs which they invest. Bichat frequently examined the spleen of dogs. He detached it from some of its adhesions, and left it protruding from the wound in the abdomen "to study the phenomena;" and he saw "themselves tearing off that organ, and though in a state perfectly calm, eating it, and thus feeding upon their own substance." In some experiments, in which part of their intestines were left out, he saw them, if left loose for a few minutes, tear them without any visible pain.

Except under Disease.—Although it may be advantageous that these important organs shall be thus devoid of sensibility when in health, in order that we may be unconscious of their action and motion, and thus they may be rendered perfectly independent of the will, yet it is equally needful that, by the feeling of pain, we should be warned of the existence of any dangerous disease: and thence it happens that this membrane, and also the organ which it invests, acquire under inflammation the highest degree of sensibility. The countenance of the horse labouring under pleurisy or pneumonia, will sufficiently indicate a state of suffering; and the spasmed bend of his neck, and his long and anxious and intense gaze upon his side, tell us that that suffering is extreme.

Yet not under every Disease.—Nature, however, is wise and benevolent even here. It is not of every morbid affection, or morbid change, that the animal is conscious. If a mucous membrane is diseased, he is

rendered painfully aware of that, for neither respiration nor digestion could be perfectly carried on while there was any considerable lesion of it; nay, the animal often expresses a great degree of pain, plainly referable to these membranes, and yet, on examination after death they exhibit little or no morbid change: but on the other hand, we find tubercles in the parenchyma of the lungs, or induration or hepatisation of their substance, or extensive adhesions, of which there were few or no indications during life.

The adherent Surface of the Pleura.—In some few cases (and the thorax furnishes us with one) the pleura is isolated on both sides. It is so in the mediastinum, and then both sides present the same polished glistening appearance; but a serous membrane is generally an investing one, and then it has a very different surface internally, namely, a roughened floculent one, by means of which it adheres so closely to the part beneath, as to be scarcely separated by the most careful dissection. The pleura adheres intimately to the ribs and to the substance of the lungs; yet it is a very singular connexion. It is not a continuance of the same organisation; it is not an interchange of vessels. The organ and its membrane, although so closely connected for a particular purpose, yet in very many cases, and where we should least of all suspect it, they have little or no sympathy with each other. Pneumonia will sometimes exist, and will run on to congestion, or even ulceration, while the pleura will be very little affected, and, much oftener, the pleura will be the seat of inflammation, it will be attended by increased exhalation to such an extent as to suffocate the animal, and yet the parenchymatous substance of the lungs will exhibit little other morbid appearance than that of mere compression. The disease of a mucous membrane spreads to other parts—that of a serous one is generally isolated.

The Continuity of Texture.—It was to limit the progress of disease that this difference of structure between the organ and its membrane was contrived, and the more to ensure this, the serous covering of every viscus, abdominal and thoracic, is perfect. The pleura is no-where perforated even to give passage to the smallest blood-vessel or nerve, but they enter at one point only, and between a duplicature of the membrane. We see the reason of this. I have now immediately exposed to observation the lungs—a passive organ—a mere mass of cellular texture and innumerable vessels—and close by it, or rather between its lobes, I find the heart—an active organ, always and energetically at work—a mass of muscle, irritable and powerful. Their structure is altogether different—their function is as different—they have nothing in common, except that the blood traverses both, but for a totally opposite purpose. They may interfere with and confuse the duty of each other,—and the one can scarcely exert an advantageous influence over the other; then observe, although in approximation with each other, how perfectly they are separated. The double serous investment of the heart, the serous investment of the lungs, with the additionally interposed mediastinum, and this investment on each through its whole extent impervious, render the one organ in a manner independent of the other, and are contrived for this especial purpose. We shall see by-and-by, that the horse may have, or may perish from, carditis, and the lungs will exhibit no appearance of disease; and, on the other hand, he may be destroyed by pneumonia, or from pleurisy, and the heart will scarcely sympathise more than any other part of the frame.

The independence of the Serous Membranes.—Again, do me the favour to observe, that between the heart and the mediastinum, in the folds of which it is concealed, there is no communication even by the slightest filament. The mediastinum is the continuation of the investing tunic of the lungs,—the pericardium of that of the heart. They are in contact with each, but they are as distinct and unconnected as if they were placed as distant from each other as the frame would admit. Is there no meaning in all this? Have we been sufficiently aware of the important object thus effected? It is to preserve the perfect independence of organs equally important, yet altogether different in structure and function;—to oppose an insuperable barrier to hurtful sympathy between them, and especially to cut off the communication of disease.

The Effect of Purging in Pleurisy and Pneumonia.—I must not too much anticipate, but perhaps a little light begins to be thrown on a circumstance of which, I hope, you will not have too frequent and painful experience. I mean the fact, that while we may administer physic, or mild aperients at least, in pleurisy, not only with little danger, but with manifest advantage, we may just as well give a dose of poison, as a physic-ball to a horse labouring under pneumonia. The pleura is connected with the lungs, and with the lungs alone. There is not a connecting filament with any other viscus, and although connected with the lungs, the organisation is so different, that there is very little sympathy between them. A physic-ball may therefore act as a counter irritant, or as giving a new determination to the vital current, without the propagation of sympathetic irritation; but the lungs or the bronchial tubes that ramify

through them, are continuous with,—it is a somewhat round-about way,—but they are continuous with the mucous membranes of the digestive as well as all the respiratory passages; and on account of the continuity and similarity of organisation, there is much sympathy between them. The irritation caused in the one is easily propagated to the other; or if there be irritation excited at the same time in two different portions of the same membrane, it is probable, that instead of being shared between them, the one will be transferred to the other—will increase and double the other, and act with fearful and fatal violence. Do not you, Gentlemen, require too many lessons of this kind to teach you caution.

The Mode of Investment.—A few words, and these in the simplest and perhaps superficial way, must now be said as to the manner in which the thorax and lungs are covered by the pleura. I take away a portion of the wall of the thorax, and by careful dissection I arrive at this membrane, forming the inner lining of that portion of the ribs which I am removing. I cut through this membrane and introduce my hand into the chest, and I feel the smoothly polished pleura covering the bony wall of the thorax above and below from the spine to the sternum, and before and behind from the first rib to the diaphragm; but when I attempt to pass my hand beyond the spine or the sternum to the other side of the chest, I meet with an insuperable obstacle; the pleura is leaving the spine above and is reflected or turned downwards, and it is also leaving the sternum below, and is reflected upwards,—it is leaving the diaphragm, and I can trace it forwards, and it is quitting the first rib, and I can trace it backward. I follow it, and I most conveniently follow it from below upwards. I can trace it all along the sternum, reflected upwards, and I can trace a portion of it both anteriorly and posteriorly as high as the spine, but centrally, and introducing my hand between the lungs, and directing it towards the spine, progress is opposed. There is a voluminous mass, round which, however, I can plainly feel the pleura turning. On examination I find this to be the origin of the bronchial tubes, or the bifurcation of the trachea on either side, together with the vessels proceeding from the heart to the lungs, and from the lungs to the heart. These to a certain extent are covered by this reflexion of the pleura, and closely embraced by it, and then examining more closely both the outer and inner surface of the lung, I find that the pleura dilates, and forms a kind of bag, and spreads over and contains the whole of that lung.

Two Pleuræ and the Lungs between them.—On the other side I find precisely the same structure, except that the reflexion from the sternum always takes place a little on the left of the mesian line. Then I have two distinct pleuræ, each lining the ribs and a part of the diaphragm, and constituting the wall of partition along the thorax, and including the lung in a fold or prolongation of itself; and I have two distinct cavities, the right being rather the larger, because that lung is of somewhat greater size than the left. I have also this singular fact, that the lungs are, in one sense, as much out of the pleuritic cavity as are the bony parietes of the thorax, and are covered like the walls of the thorax by the outer surface of the membrane. The sacs are empty, and the lungs are curiously enveloped in folds, or dilatations of one of the sides, and in contact with the outside of the bag and not the inside. There result however, from this singular arrangement some important circumstances which could not otherwise have been accomplished; the polished and glistening surfaces of the pleuræ are opposed to each other, and the utmost freedom of motion is thus permitted; a duplicature of membrane is interposed between the lungs and their bony wall, and a closed cavity is formed in which there may always exist a moisture or dew, to obviate injurious friction, to prevent concussion by its interposition; and to be contained comparatively harmlessly there, until it can be removed by the absorbents, when from cause of irritation an undue quantity has been poured out.

The Mediastinum.—The reflexion from the sternum upward and the spine downward, is called the mediastinum. It evidently consists of a duplicature of pleura, or the unattached or reflected membrane on each side, and between these membranes we find the heart securely lodged; but its situation and connexions and the advantages derived from them belong to another system. That portion of the mediastinum anteriorly which is not thus occupied is called the *anterior* or *little mediastinum*, the contents of which have been already described; and behind is the *posterior* or *great mediastinum*, containing the gastric portion of the œsophagus, the posterior aorta, the posterior cava, and a kind of fold in which is wrapped the larger of the two lobules of the right lung.

We are now prepared to return to the intimate structure of the lung itself, but this is an important subject, and deserves a separate lecture.

EXTRAORDINARY INSTANCE OF

STRICTURE

AT THE

SIGMOID FLEXURE OF THE COLON,

IN A FEMALE.

With a Communication between the Intestine and Bladder; the Faces, &c., passing by the Urethra.

By FREDERICK SALMON, Esq., *Surgeon, London.*

Mrs. B., age 59, March 17th, 1831.—I was requested to see this patient by my friend Mr. Coward, of Hoxton Square. Upon my first interview with her she gave me the following narrative of her sufferings:—

"For some years I have been subject to irregularity of my bowels, which have more usually been loose than confined, and occasionally, though not often, I have had a good deal of blood pass with my motions. For the last two years I have had frequent attacks of purging, my bowels oftentimes being opened twelve and even twenty times in a day, though I did not suffer much excepting at the entrance to the passage, at which place I have had a good deal of smarting, pricking pain, and there, for the last twelve months, there have been some small lumps, which are much larger at one time than another. My water for many months past has been very troublesome, and I am now obliged to pass it every hour or so, unless I am lying down, when I can retain it for two or three hours together. About eighteen months ago I was seized with inflammation of my bowels and retention of urine, from which I nearly lost my life. A month or six weeks after this attack I had a violent purging, which lasted for three or four weeks, from which time to the present day I have never voided anything like a figured stool. When I go to motion I have a bad griping pain over the centre of my belly, at the lowest part, and in my left groin, which lasts for ten minutes or so, when my evacuations come away with a jerk, and are fluid, like an infant's. About nine months ago, after one of my attacks of purging, I thought a part of my motion came away in my water, which I mentioned to a medical man, who assured me it was all a pack of nonsense. In a few weeks I was certain it was the fact, as I used to find the stool lodged in the front passage every morning when I got up. In January last I first sent for Mr. Coward."

I found this poor creature suffering from pain in the region of the sigmoid flexure of the colon and the bladder, extending into the rectum and vagina. She had a frequent inclination to make water, and her bowels were relieved upon an average ten times in every twelve hours; she complained that at the period of passing her stools the scalding and pain in the fundament almost distracted her; she also suffered much uneasiness in the front passage, particularly when she began to pass her motion, but as her bowels were emptied, this generally subsided. The stools did not always come the wrong way, but appeared to depend on the looseness of her bowels, the quantity which passed per vaginam, increasing or diminishing according to the relaxed or confined state of her body. To my question, Are you certain that your motions come away by the front passage, she answered, "O yes, my nurse took away as much as a small teacup-full this morning; but it does not happen every day alike; some days scarcely any at all comes; at others it will pass, if my bowels are very relaxed, at every motion." I asked her if it ever passed with her water, she replied, "It did not now, but that this *was* very bad, for she could not retain it more than an hour or two at the most." Her general health was not materially impaired, neither did her appearance indicate any serious indisposition. She was engaged in her domestic occupations, and able to assist herself as heretofore, occasionally taking exercise, and, during the intervals of her bowels being relieved, performing her usual family duties. *No examination of the rectum had ever been made.* I advised,

Hyd. c. creta, gr. *sex*;
Pulv. opii, gr. $\frac{1}{4}$; *omni 3tia horâ. Enema opia nocte maneque.*

18. She was more tranquil, and had had a better night than for many weeks; her bowels had been relieved only three times during the last twelve hours.

Cont. med. omnia.

20. She had passed very little motion by the wrong way, her bowels had only been acted upon twice in twenty-four hours. She complained of feeling drowsy and stupid.

Omit. pulv. opii, hyd. c. creta gr. sex omni nocte. Enema omni mane. Milk diet, with rice-milk.

25. She had been very comfortable; her bowels had been quiet, and had acted this day without any motion coming the wrong way, and she had but little irritability of the bladder. I this day examined the rectum, and discovered a stricture at the sigmoid flexure of the colon, through which I passed a No. 5 rectum bougie. The examination gave her no material pain, though she felt a kind of burning heat in the passage, just the same as when she passed her stools. I could not discover any disease in the rectum, beyond two or three trifling hemorrhoids. She retained the bougie without

any annoyance for four minutes. Upon withdrawing it the stricture was extremely well delineated, a portion of the instrument, about an inch in length, being much corrugated, where it had lain in the contracted part of the colon.

Cont. ut antea.

28. She had experienced no pain from the examination. On the contrary, her motions passed easier. I requested her to allow me to examine the vagina, but she begged I would delay doing so for a few days.

Con. ut antea.

April 5. She had suffered great pain and difficulty in making water all night, which passed by drops, with an acute burning sensation. No motion had passed by the front passage for the last two days.

Oleum ricini, ʒii, *hac nocte.*

℞ *Mist. mucilag.*, ʒvi;
Spt. æth. nit. ʒii;
Tinct. opii, ʒi. *Misce. Sum. coch. duo magna 4tis horis.*

6. Urine passed more readily, but with great pain. Her bowels had acted freely, but with much uneasiness to her. I introduced a No. 6 rectum bougie without much trouble, and she retained it with facility nine minutes.

Discen. mis. mucil., et hyd. cum creta.

From this period to the 26th, she went on much as heretofore; some days she suffered but little inconvenience in the urinary organs; on others she would be compelled to pass her water every ten minutes; a very trifling quantity of motion had passed by the front passage. On the 26th she assented to my making an examination per vaginam; but though I made the most cautious investigation, I could not discover any communication between the rectum and this part. Her health, within the last week, had not been so good; she had lost her appetite, slept badly, and had an irritating cough; she was likewise unable to sit up the whole of the day, being compelled to go to bed by six o'clock.

Cont. ut antea.

May 7. She had gone on much in the same way, though, upon the whole, she did not suffer so great a degree of irritation in her bowels, but her desire to make water was greater than ever, and it flowed from her whenever she coughed violently. I introduced a No. 7 bougie, which she retained for five minutes; when I withdrew it, she complained of its causing her a burning pain down the back passage; the upper half of the instrument was covered with a most offensive discharge. I requested her to permit me to examine the vagina with the speculum, but she would not.

Hyd. c. creta, gr. v, *omni nocte;*
Pulv. opii, gr. i, *p. r. n.;*
Enema opii, ʒii, *omni mane.*

17. She had been rather more tranquil for the last few days, yet her bowels continued irritable, and she usually passed some small quantitity of motion by the front passage daily. A good deal of blood had also passed by the rectum. No alteration in treatment.

26. Her motions had become very offensive, and contained a great deal of matter; the bladder was extremely irritable. Thinking this excessive irritability might be produced from some accumulation above the stricture, I gave her a brisk purgative.

27, 28, 29. She had rested better, and had been more quiet in her bowels than for the last five weeks, but her motions usually contained more or less blood and pus. With these symptoms I did not consider it prudent to attempt the use of the bougie.

June 2. The irritation of her bowels had returned as badly as ever, and she felt a cutting pain in the left groin whenever she went to stool. It was now evident that I could do nothing for this poor creature, since the irritability of the bowel prevented the use of instruments for the purpose of dilating the obstruction; nor could I understand how the motions passed per vaginam, being unable, upon the closest examination, to discover any communication between the anterior and posterior passages.

It now, for the first time, occurred to me that the stools *might* pass per urethram, but my patient induced me to discredit this opinion, because she said she was sure the aperture was at the top of the upper and back part of the passage.

Mist. cretæ c., ʒvi;
Tinct. opii, ʒii. *Misce. Sum. coch. duo magna 3tia quaque hora, suppos. opii rec. hora somni.*

9, 18, 21, 25, 28. No material alteration took place between these dates, on each of which I visited her. Last night, while she was retching on the close stool, she felt a pricking kind of pain in the front passage, as if some substance was sticking there, and being convinced that such was the fact, she asked her female attendant to put her finger to the spot, when she felt a hard substance fixed in the passage, which she brought away with a pair of pincers. *It was a triangular piece of bone*, which she had accidently swallowed.

I now entertained slight hopes that I might be able to do some good in the case, considering it possible that the irritation might have resulted from this substance

being lodged in the stricture. With this view I resolved to keep her under the influence of opium for a few days, so as, if possible, to quiet the bowels and bladder.

July 5. She was so much better, that she was able to sit up and enjoy her meals, and even to do trifling matters about the house herself, nor had she had any discharge from the front passage worth mentioning. —To prosecute the same plan.

7 and 9. She continued to improve in the bowels, and this day passed a small figured stool, which she had not done for the last eight months, the bladder was however very irritable. I examined the urine, which presented no remarkable appearance.

11. Another attack of irritation of the bowels had come on last night, and she had passed a great quantity of blood and matter per anum, but voided little motion by the front passage.

℞ *Infus. quassiæ*, ℥i ;
Cupri sulph., gr. ¼ *6tis horis* ;
Pulv. opii, grs. ii, *hac nocte*.

12, 14, 16, 19, 20. The irritation of the bowels continued more or less. She complained of great pain in her back, and in the region of the bladder.

℞ *Pulv. trag. comp.*, ℥i ;
Nitri, grs. vi ;
Opii, gr. i. *Misce. Ter die sumendus. ex aqua hord. Fotus regioni lumb.*

22. Her urine had passed pretty freely for the last twelve hours, but, said she, "the torment I suffer from my bowels no one can tell."

Her strength was now evidently fast declining, she was unable to leave her bed, the stomach was irritable and rejected her food, although she took scarcely anything. In fact I gave up the case as hopeless. She lingered on in the same distressing way, becoming daily worse and worse, till at last she was obliged to be kept constantly upon the bed-pan, owing to the excessive irritability of the rectum, from which either an offensive discharge, or feculent matter, kept constantly oozing for the last fortnight of her life.

On the 27th of August, death relieved her from the most miserable state of suffering I have ever witnessed. I saw her daily; she was sensible to the last, though, with a hope of lessening her sufferings, she took a grain of opium every hour for the last fortnight of her existence. Several times prior to her death, which the poor creature contemplated with the happiest serenity of mind, she requested me to promise that I would examine her body, her only grief being, as she said, "that she should not be able to be satisfied of the true nature of her disease."

Sectio Cadaveris.

Aug. 29, two P.M. Though there was much general emaciation of the body, the fat over the parietes of the abdomen was of considerable thickness. Upon making the usual incision through the abdominal muscles, the peritoneum covering the rectus was found closely adherent to the sigmoid flexure of the colon, the adhesions being firm, and evidently of some standing. The membrane at this part, and in that portion of it which was reflected upon the bladder, and over the lower part of the colon, was thickened in texture, and of a dark-green colour. The descending colon, sigmoid flexure, and that portion of the rectus muscle which was agglutinated to it, the rectum, pelvic viscera, together with some of the soft parts around the anus and meatus externus, were then removed from the body. There was not any disease of the rectum worthy of notice, and after examining the bowel with the greatest care, no communication could be discovered between it and the vagina. The latter passage laid open, presented no morbid appearance of moment, being only trifling inflamed. The uterus was likewise healthy, as far as its *general* structure was concerned, but a portion of its cavity towards the fundus was obliterated, from coagulated lymph having become organised in several bands. A very small polypus grew just within the os tincæ. The urethra was next laid open into the bladder; this canal was much dilated and vividly inflamed. The bladder, notwithstanding the destruction of a part of its fundus, was not generally diseased, nor was there any material inflammation or thickening of its internal surface; but the colour of this coat was of the peculiar green cast before alluded to; it was much contracted and perfectly empty.

An incision was now made through a part of the descending colon, its sigmoid flexure, and the first curve of the rectum, so as to bring into view the extent and nature of the stricture. Immediately above the sigmoid flexure, the colon was distended, and contained a considerable quantity of fæces, but at this part the intestine was so contracted, that the point of the fore-finger could hardly be passed through it.* About three inches of the bowel were adherent by the superior surface to the peritoneum covering the abdominal muscles, while the inferior portion of the intestine was in like manner firmly united to the fundus of the bladder, a portion of which was destroyed by ulceration, together with part of the colon to

* The contraction was circular in form, and about an inch in length; the mucous membrane of the intestine immediately before, throughout, and for some short distance beyond it, being destroyed by ulceration.

which it was united, so as to form a direct channel for the passage of the feculent matter. There was not so much thickening of the intestine as might have been expected, and the ulceration appeared as if it had been produced more from acute inflammation, than any chronic disease of the part.

The viscera of the chest and abdomen were healthy.

12, Old Broad Street,
March 15, 1832.

SUPPOSED DISCOVERY BY PROFESSOR DELPECH OF AN AFFECTION OF THE SEMILUNAR GANGLIA IN THE MALIGNANT CHOLERA, SHOWN TO BE ERRONEOUS.

To the Editor of THE LANCET.

SIR,—The recent researches of Professor Delpech in Musselburgh and Glasgow, would seem to point at some affection of the semilunar ganglia and solar plexus, as the proximate cause of the disease called cholera. From the appearances observed by him, he has also drawn certain physiological deductions, which, if not erroneous, at least rest on very slight foundation. In presuming to question the anatomical accuracy of the investigations conducted by Professor Delpech, I may seem presumptuous, and therefore feel it necessary to say thus much of my pretension to offer an opinion. Having been for some years an anatomical assistant to the professor of anatomy in the University of Dublin, and one of his demonstrators in the dissecting-room, my attention was particularly directed to the anatomy of the nervous system, and I have more than once dissected the whole of this portion of the body, as the groundwork of a treatise, now in course of publication, on the Anatomy of the Nerves I may therefore presume to say, that the natural appearance and situation of the semilunar ganglia are familiar to me. On Monday, the 5th inst., I was requested by one of the district inspectors to examine the bodies of two persons who died of cholera. There were present several distinguished members of the profession in this metropolis, with the professor of anatomy to the College of Surgeons in Ireland, Professor Delpech, and Dr. Læwenhagen of Moscow. The cavity of the thorax having been examined by me, the knife was resigned to Professor Delpech, that he might demonstrate the solar plexus and semilunar ganglia himself. The professor first exposed a branch of the pneumo-gastric nerve proceeding from the coronary plexus towards the transverse fissure of the liver; it was quite healthy in appearance, but M. Delpech affirmed it was enlarged. I cannot take upon me to say that the individual nerve then displayed was not enlarged, because the original size of the branch was indeterminate, but may with confidence declare that I have many times seen as large branches proceeding to the hepatic plexus, and therefore would conclude the nerve alluded to healthy. The professor of Montpellier, to satisfy all scruples, now proceeded to scrape through the hepatic plexus with the point of the knife, and soon came down upon a roundish dark body, situate a little to the right, and *above* the vena cava ascendens. It was surrounded on all sides by the filaments of the solar plexus. This M. Delpech pronounced to be the right semilunar ganglia, and after having drawn the attention of all present to its external appearance, which was very dark, and conveyed the impression that the ganglion was much swollen, he cut into the substance; the internal appearance of this body was even more extraordinary; it was a mixture of dark and yellow, and the substance seemed very soft; the point of the knife being applied to the divided surface, a portion readily came away, and the ganglion itself pronounced in a state of ramollissement. The substance was shown to Professor Harrison, who shook his head, and said it was very extraordinary. I felt convinced, from the superficial situation of this body, that it could not be the semilunar ganglion; but not venturing to impeach the accuracy of M. Delpech, I determined to find the "veritable" semilunar ganglion in the other body which lay on the table. The same idea seemed to have passed through the mind of Professor Harrison, for he threw off his coat, raised up the renal capsule with his finger, and tearing away a little cellular substance, in a few seconds exposed the right semilunar ganglion with the great splanchnic nerve, nearly an inch and a half below the substance which M. Delpech had divided, which was now sought for, and discovered to have been a lymphatic gland. The true semilunar ganglia of both bodies were healthy, the cellular substance surrounding one of them was certainly much injected, but the ganglion was neither inflamed nor altered in any way, and the injection equally existed in the cellular membrane of all the neighbouring parts. I have seen the same kind of injection in the left semilunar ganglion of a subject examined since, but the most careful scrutiny could not detect any alteration in the interior of the ganglion. From the very confident manner in which M. Delpech pronounced the ap-

pearance of this gland to have been the same as those which he had before witnessed, and which he then expected to find, I cannot avoid concluding that he has fallen into a similar error in some of his former dissections.

The physiological reflections appended to M. Delpech's paper, in the last Cholera Gazette, rest on the supposition that the actions and functions of the arterial system depend on the influence of the sympathetic nerve. If we examine for a moment what evidence we have in support of the proposition, we shall find it a mere gratuitous idea. Anatomy demonstrates that the filaments of the sympathetic accompany the arteries through a great portion of the body, but we have no experiments or observations to prove what influence the sympathetic has on the arterial system, or whether it have any at all; in fact we might freely confess that we are entirely unacquainted with the physiology of the sympathetic nerve. In the fœtus its rudiments are not seen until the third month, long after the development of the blood-vessels; and some fishes, as the squalus, raja, &c., have no sympathetic nerve at all. Any conclusions, therefore, drawn from such premises, must at least be received with hesitation. That pathological anatomy will render material assistance in our researches after the efficient cause of this disease, I have no doubt; but in order to be useful, it must be prosecuted with great diligence and care. Already many supposed causes have been eliminated by our post-mortem examinations, and the number of tissues essentially engaged in the disease are reduced to a very few. We have also been enabled to discriminate between the accidental and the necessary morbid appearances, and thus in future will employ ourselves with the greater advantage in prosecuting the inquiry into the primary tissue or organ affected.

The preceding observations, Sir, will show how careful even the greatest men should be in accuracy of examination, and are, at your request, submitted to the public. I am, Sir, your obedient servant,

P. H. Green, A.B., M.B.

London, March, 1832.

CASE OF

MALIGNANT CHOLERA

SUCCESSFULLY TREATED IN THE INTENSE STAGE.

To the Editor of The Lancet.

Sir,—I take the liberty of forwarding to you the accompanying minutely detailed case, requesting room for it in your next number. It appears to me to show the necessity of most assiduously employing our remedies in similar cases, as well as the efficacy of the plan adopted, which in my estimation has been successful, chiefly from the stimulants used,—infusion of cayenne pepper and ammonia. Bleeding and the other remedies I have used in many similar cases which have terminated fatally. In this case also no trace of contagion is discoverable, nor has there been any in the many other cases which have occurred to me within the last month. Since detailing this case, a similar one has happened to me, similarly treated, with the like result.

I remain, Sir, your obedient servant,

Joseph T. Mitchell,
One of the Parochial Surgeons of Lambeth.

Harleyford Place, Kennington Common,
March 15th, 1832.

CASE.

James Perry, ætat. 45, No. 4, Hunt Street, George Street, Vauxhall, bargeman on the Thames, of spare habit, short stature; usually had enjoyed good health; of sober habits, scarcely ever drinking anything but water; had lived well, having generally dined on animal food four or five days in the week; had constantly enjoyed most of the comforts of life, warm clothing, and a warm bed. Being a man of good character, he was valued by his master, Mr. Stapleton, of Whitefriars Wharf, in whose employ he constantly was. Left Whitefriars on Thursday, March 1st, 1832, in the barge; arrived at Raynham, in Essex, in the evening, same day. Next day was attacked with slight relaxation of the bowels, which continued to increase in severity daily, but gradually, until Friday morning, March 9th. Left Raynham on the 6th; his barge hung on a collier in the stream for a short time to wait for tide; did not board her. Returned to Whitefriars' Wharf, thence to Charlton on the 7th, then on the 8th went to Mill Bank, Westminster, and put the barge into dock in the evening, after which he returned to his home. During this period he had not been near any infected house or ship. His diarrhœa had existed five days before his barge hung on the collier. His brother bargeman not at all affected. Had not taken anything that he knew of that could have deranged his bowels. Friday morning at three o'clock, seized with violent spasms of the thighs and legs, which continued incessantly until 10 A.M., the purging then having become most violent. Stools of the appearance of rice gruel, very watery and abundant; purging incessant until ten o'clock. Body universally cold; extremities icy cold; countenance sharp and of a leaden hue; eyes much

sunken; voice peculiarly shrill (*vox cholerica*). The last few stools had not been thrown away, and on examination I found them to present the appearance of rice gruel, with feculent white matter of an albumenous nature floating in it. Had only vomited once, and that at half-past nine; I saw him at ten and gained the above information from his wife's sister. The spasms in the thighs and legs were then most violent. His hands and arms and feet cold as ice, and of a blackish livid purple colour; countenance as described; body universally cold; pulse extremely low, sometimes scarcely to be felt at the wrist, about 80 in the minute. Had passed no urine that the bystanders knew of in the night. My mind was quite made up that he would die. I therefore prepared his relatives to expect such an event, as I had seen several other cases with precisely the same symptoms, which were no more severe than those in this case. Gave him a table-spoonful of flour of mustard, and a table-spoonful of table salt in half a pint of warm water, followed by half a pint more warm water; tied up his arm, and when the emetic began to operate, opened a vein from which I obtained twelve ounces of very black and thick blood, which when coagulated showed but little serum. Applied a mustard poultice down the spine, from the nucha to the sacrum; gave him after the emetic had operated and the bleeding was over:—

Hydrarg. submuriatis, gr. v;
Opii, gr. iij;
Hydr. submur., gr. ij, *et*
Opii, gr. ss; *2a quaq. horâ, cum* ʒiss *misturæ seq.*;

℞ *Piper. cayenneis*, ℈j.
Aq. Ballient. Oss. infundi per horam et cola, tunc adde liquoris ammoniæ carb. ʒiv;

(about gr. viij carb. ammon. for a dose), with a tablespoonful of brandy in ʒss of very *hot water* in the intermediate hour, and a quarter of a pint of hot beef-tea every hour. Covered him with hot blankets; applied hot sand-bags to the extremities; arms and legs to be rubbed with hot flannel in a direction to the heart; injected into the bowels three pints of *hot water* with ʒiss of brandy. Purging ceased, and spasms gone, after the bleeding and first dose of calomel and opium. In three hours extremities a little warmer; pulse a little more distinct; lividity much less; countenance more natural; voice still peculiar. Injected water came away; repeated the hot water and brandy injection. *Continue the mixture, 2a quaq. horâ et pill. 4a quaq. horâ.* Mustard poultice to remain on; renew the hot sand.

Seven P.M. Bowels not open since the bleeding; no spasms since; extremities warm; lividity quite gone; countenance natural; voice natural. No urine passed; pulse much stronger, and regular. He appears now in the state of a patient in collapse in typhus fever. Congestion appears about the head. Calomel and opium every six hours; the mixture of capsicum every three hours. Continue the brandy and hot water; the hot-water injection renew.

10th, ten A.M. Symptoms all better, much rallied; yet still great disposition to sleep, and appearance of congestion in the head; applied six large leeches to the forehead. Omit the calomel and opium; repeat the mixture every four, five, or six hours.

Ten P.M. Congestion about the head much relieved; animated; had passed no urine until to-night for 43 hours.

11th. Convalescent; no subsequent fever; urine natural; had passed a loose stool, which appeared to be beef-tea.

12th. Had passed three loose feculent stools. Recovering, yet weak.

13th. Gums tender from the calomel.

ON THE

SULPHATE OF INDIGO

AS A

TEST FOR NITRIC ACID.

To the Editor of THE LANCET.

SIR,—Were I not conscious of the truth of what I am about to advance, I would not on any account encroach upon your valuable pages; but as you have always shown a wish to publish whatever might benefit the public in general, and medical practitioners in particular, I hope you will allow me a column of your journal for the publication of what I consider entitled to some attention, which I will endeavour to make as concise as possible.

I have lately gone over the interesting experiments of Dr. O'Shaughnessy on nitric acid, and I am glad to find that this acid, from being one of the most difficult to test in cases of medical jurisprudence, has become, comparatively speaking, one of the easiest. But in two or three instances I am compelled to differ from him. The first is in the formation of the detonating cyanate of silver: he observes, that white flakes begin to fall within the space of three minutes after the mixture has attained to the boiling point. Now out of more than thirty experiments, repeated exactly as is laid down in the text, I never once succeeded in less than a quarter of an hour. Generally I was above half an hour, and five or

six times not at all. I will not say that this is an exaggeration on the part of Dr. O'Shaughnessy, but we must all allow, that in the hands of a youthful experimentalist it might never succeed, or otherwise he might not have patience to persevere. Add to this, the mixture must contain more pure nitric acid than is specified, for instance, three drops. Such being the case, the test itself, I presume, is hardly deserving the commendations the above toxicologist has been pleased to pass upon it, as from my own observations it cannot be employed when the acid is moderately diluted.

The next point to which I have an objection, is in the formation of nitrate of urea. This, he says, may be formed by adding the smallest drop of nitric acid to urine, evaporated down to the consistence of syrup. Such is precisely the case; but on coming to the conclusion of the passage, he observes that oxalic acid has the same effect. Now, Mr. Editor, do not suppose I am ignorant of the abilities of the man whose paper I am now criticising; I have perused his publications too attentively, and followed him through all the intricacies of his experiments too closely, to doubt for a moment his profound skill, whether in the laboratory or elsewhere, but for all this I will not yield to him one iota of what I have alleged; I am too well aware of its exactness to give up the point so readily. That oxalic acid produces the same change, I deny; for out of as many experiments performed with that test as were made with the cyanate of silver, I did not succeed once. Again; diluted nitric acid, in this instance, is of no utility whatever; therefore I should conceive that urine would be of little service in cases of poisoning, unless some portion of the undiluted acid by which the deceased was destroyed could be procured, which however I believe is not often the case.

I now come to the third and last point, which is in the instance of sulphate of indigo. Dr. O'Shaughnessy has entered into a long argument on the inutility of this preparation as a test for nitric acid, and from the almost insuperable evidence which he has advanced in its support, it may appear presumptuous in me to doubt it; but encouraged by what you have observed in your introduction to the practical commentaries on Dr. Christison's Processes for Detecting Poisons, I have determined, though not without mature consideration, to come forward and say a word or two in support of the old established test. I was rather surprised, on reading your review of Dr. Turner's Chemistry, to find that you were in perfect concordance with Dr. O'Shaughnessy, blaming the former gentleman for not correcting the error which had duped chemists for so long a time, by blotting from his valuable pages that test, the utter fallacy of which had been so indisputably established. It appears to me, however, that Dr. Turner knew too well the strength of the evidence on which he grounded his assertion, to concede what the generality of toxicologists knew to be a fact, to that which, comparatively speaking, had been no more than suggested. May I ask you, Mr. Editor, whether you have repeated the experiments in question, and whether you have come to the same conclusions as Dr. O'Shaughnessy, or you have, omitting that very useful method which you have followed with Dr. Christison, trusted to the profound knowledge and high reputation of your correspondent, and with your accustomed consciousness of truth, based what you were advancing on the words of another?

Excuse this deviation; I will now come directly to the point, and in order to expose the falsity of Dr. O'Shaughnessy's experiments, I will just beg leave to quote the paragraph on this head from Dr. Turner, and then contrast it with that of your correspondent.

Dr. Turner observes, that a new test of the presence of nitric acid has recently been proposed by Dr. Liebig: the liquid to be examined must be mixed with a sufficient quantity of a solution of the sulphate of indigo in sulphuric acid; for acquiring a distinct blue colour a few drops of sulphuric acid to be added, and the mixture boiled. If a nitrate is present the liquid will be bleached, or if the quantity is very small, rendered yellow; by this process nitric acid may be detected, though diluted with 400 times its weight of water; or by adding a little muriate of soda to the liquid before applying heat, one five-hundredth part of nitric acid may be discovered.

So much for Dr. Turner, now for Dr. O'Shaughnessy. He says, "A few test tubes were filled to the same height with water, and the fluid tinted to the same depth of shade by the blue solution of indigo in sulphuric acid. Dr. Liebig's process with the nitric acid was performed on the first, and the colour was immediately discharged as he describes. To the fluid in a second tube a few drops of sulphuric acid were added, as Dr. Turner desires in the process for nitric acid, and the colour, on the mixture reaching the boiling point, was discharged as effectually as in the first experiment. A third trial was made with muriatic acid alone in the same quantity, and the tint was again destroyed; and in a fourth experiment two grains of the muriate of soda, with one minim of the sulphuric acid, produced the same effect."

These experiments I repeated almost

times innumerable, and every trial gave additional proofs of the truth of what I had before observed. I poured into each of five test tubes about half a drachm of water coloured lightly with the blue solution of sulphate of indigo; this done, I let fall into the first one drop of nitric acid, into the second the same quantity of muriatic acid, into the third as much sulphuric acid, into the fourth two grains of muriate of soda, with one minim of sulphuric acid, and into the fifth as much nitrate of potash and sulphuric acid as the muriate-of-soda solution contained. Then I applied each successively to the flame of a spirit-lamp, and after boiling about half a minute, the colour of the nitric acid and nitrate of potash mixtures was entirely dispersed, leaving the liquid of a fine straw colour. That which contained the muriatic acid lost a little of its tint when heated, but quite sufficient remained to distinguish it from the nitric acid. This is shown to more advantage by bringing them to each other, and placing the tubes on a dirty napkin. The other two were not in the least affected. Often as I tried these experiments, the blue colour was always discharged by nitric acid on the mixture reaching the boiling point, and even without heat, providing enough acid had been added. I see no necessity for the drop of sulphuric acid being made use of, as proposed by Professor Liebig, as it has no perceptible effect upon the liquids, nor can I conceive how it should, when the indigo itself is dissolved in that menstruum, even concentrated. It contradicts the facts which, by the theory of Dr. O'Shaughnessy, it attempts to establish; or why is not the blue colour lost in the formation of the sulphate of indigo itself? for there a very moderate heat is applied. Your correspondent has remarked that the solutions ought only to be coloured very lightly, or the change will not take place; but I would add, that such is not the case with nitric acid, for however dark or however light the colour, providing the acid be in proportion, the shade is discharged; whereas the sulphuric and muriatic acid mixtures may be but tinted with the blue test, and the acids themselves added in large quantities, and yet, on being heated, little or no decolorisation takes place.

But granting what Dr. O'Shaughnessy alleges, would not the difference of effect, in point of time, be sufficient to stamp the utility of the sulphate of indigo in this instance? In the meantime, we must not forget, that chlorine and muriate of tin possess the same properties of decolorising the indigo solution; but the effect which morphia has upon nitric acid will sufficiently distinguish the one in question from the other two; besides, the difference in their appearance, when the colour is discharged, would be enough to discover the nitric acid, could no other test be brought to our assistance.

In conclusion, Mr. Editor, I would just ask you, whether, out of the numerous trials I have given this test upon the acids, I might not by chance have succeeded once. Can you suppose it possible, that, however carelessly I tried these experiments, I could miss stumbling on the right way at least once, and thus have established the facts advanced by Dr. O'Shaughnessy? But I never did once succeed, and each experiment was performed with every possible care, and exactly in the manner laid down by your "Correspondent." Much more might be offered in confirmation of what I have already said, but I fear even now I have trespassed too long upon your time, therefore, leaving you and your readers to give the above remarks the strictest scrutiny, I conclude by subscribing myself

Your constant Reader,
JOHN S. HILEY.

Elland, near Halifax,
March 10, 1832.

LONDON PHRENOLOGICAL SOCIETY.

January 16, 1832.

Dr. ELLIOTSON in the Chair.

LIFE AND CHARACTER OF DR. PARR.

THIS evening the following paper was read by Mr. SEDGWICK on the phrenological development and character of Dr. PARR:—

At our last phrenological meeting, the Society's collection became enriched through the kindness of a member, Mr. Whitwell, by the cast of the head of the late celebrated Dr. Parr. My name standing third on the list of those who have entered into an engagement, each in his turn, either to write or provide a paper for the Society, the duty has this evening devolved upon me, and I embrace with pleasure the opportunity afforded me by Mr. Whitwell, to call your attention to some points in the history and character of that remarkable person, in connexion with his phrenological development—than which a more striking instance of the truth of our science cannot I think be adduced. It is to be remembered that our principal object is the collection of *facts*, and in this it is advisable to select well-authenticated cases, such as the present, where the character of the individual is well known, but which would

not perhaps, without the aid of phrenology, be either perfectly understood or appreciated.

I shall, in the first place, call your attention to the size of the head of Dr. Parr, both generally and locally, and then proceed to lay before you such anecdotes and

Measurements of the Cast of Dr. Parr.

From occipital spine to the lower part of individuality	8⅜
From occipital spine to ear	4⅜
From the ear to the lower part of individuality	5⅜
From ear to firmness	6
From destructiveness to destructiveness	6⅜
From cautiousness to cautiousness	6
From ideality to ideality	5⅝

It will therefore be seen that the first measurement, from the occipital spine to the lower part of individuality, exceeds the average size by six-eighths of an inch; in the twenty measurements given by Mr. Combe, only three reached eight inches, and only two eight inches and one quarter. From the occipital spine to the ear, Parr measures exactly the average size, four inches and three-eighths. From the ear to the lower part of individuality he exceeds the average size by above six-eighths of an inch, and the greatest size mentioned by Combe, by two-eighths of an inch. From the ear to firmness Parr exceeds the average size by one-tenth of an inch, the largest measurement by Combe is 6⅛, Parr six inches. From destructiveness to destructiveness, Parr measures 6⅜, which is more than three-eighths above the average size. The largest measurement by Combe is 6⅛. Cautiousness to cautiousness is six-twentieths above the average, and one-eighth less than the largest mentioned by Combe. Ideality to ideality is about the average, but when we compare that organ with the development of the other parts of the head, it must be pronounced but moderate.

events of his life as I have been enabled to collect, and which seem best adapted to display his true character and disposition, thus endeavouring to corroborate the truth of the one by the other. His head will be found to be very large; indeed greatly exceeding the average size. This remarkable size, however, will be immediately observed to arise from the extraordinary length from the *meatus auditorius* to the lower part of the organ of *individuality*; and also from the large development of the superior and middle portions of the posterior part. I do not, I must confess, like some phrenologists, place great reliance on the measurements by callipers, especially when made from the cast, but it may nevertheless be as well in this instance to give some of them, in order that they may be compared with the table of measurements from nature given by Mr. Combe, and taken promiscuously from many more in his possession.

Average Size of 20 Males, between 25 and 50, given by Mr. Combe.

From the same to the same	7⅛
From the same to the same	4⅜
From the same to the same	4⅛
From the same to the same	5⅛
From the same to the same	5⅛
From the same to the same	5⅛
From the same to the same	5⅛

The extraordinary power of learning and remembering facts, for which the Doctor was so very remarkable, depended entirely on the great length of the anterior lobes; his intellectual character originated from the length of the lower part of the forehead. His reflecting faculties by no means equal those of several others in our collection, such as Bacon, Newton, Gall, &c.; indeed, when contrasted with the breadth of forehead as exhibited in their casts, that of Parr appears comparatively narrow. I have before mentioned that the superior and middle portions of the posterior parts, form one of the most characteristic features in the head of Dr. Parr; indeed the whole of his actions may be traced to the great development of the organs of self-esteem love of approbation, destructiveness, combativeness, and attachment, which are all extremely large. Firmness is also large. Hope, conscientiousness, and indeed the whole of the coronal surface, is excellent. Secretiveness is large, acquisitiveness, moderate. Benevolence, full. Ideality and imitation, moderate. Wonder, constructiveness, and number, small. Order, moderate.

Having thus far endeavoured to lay be-

fore you the most prominent points in the phrenological development of this individual, I shall proceed to illustrate the truth of my positions by a few anecdotes, collected from the writings of his friends and biographers, commencing from his infancy.

Dr. Samuel Parr was born at Harrow-on-the-Hill, on the 26th of January, 1747. His father, to use his own words, was "a man of robust and vigorous intellect," an apothecary and surgeon of the same village. At the early age of four years he appears to have been remarkable for the precocity of his intellect, insomuch that at that period he had obtained a considerable grammatical knowledge of the Latin language. One of his biographers says he was indeed "puer animi ad præcepta rapacis," and is entitled to be placed among those who "ante annos, mentemque gerunt, animumque virilem." Such, it is said, were the displays of intellectual prowess exhibited by him in almost infantile age, on every subject to which his attention could be directed, as to call forth the loud and lavish praises of all who witnessed them. Placed upon a chair, or still more conspicuously mounted upon a table, surrounded by a listening audience of friends, he was accustomed to repeat passages from authors, or, from the suggestions of his own mind, to reply to questions proposed with a propriety and a spirit which, in a child, none could refuse to admire, and few could forbear to applaud, and not seldom, perhaps, inconsiderately or extravagantly. To this circumstance, continues his biographer, some who knew him well, have not hesitated to trace, as its first spring, that excess of vanity and self-complacency which, though a real foible even in a great character, has too often been magnified unfairly, and exposed ungenerously to public ridicule or reproach.* At the age of five he was sent as a free scholar to Harrow school, where he remained during a period of nine years. At this place several of the most distinguished political characters of the time were his contemporaries and friends; and here was first formed his ardent and honourable friendship with Sir William Jones. The *elite* of the school were accustomed to perform voluntary exercises; and an interesting detail is given in Lord Teignmouth's Memoirs of Sir W. Jones, of their manly games and principles. Under the fostering cares of Dr. Thackeray, then head master of the school, and afterwards under those of Dr. Sumner, the powers of Parr's mind soon began to open and expand, giving, as they expanded, high promise of future excellence. He was early marked by the whole school as an extraordinary boy; and was frequently accustomed to rise at five o'clock, and go into the garden with a Greek grammar for his companion. He himself often observed that his mental faculties were unfolded very prematurely; adding, too, that with him prematurity did not, as years advanced, sink into imbecility; and that early proficiency did not seduce him either into inglorious satisfaction with past, or careless indifference about future, improvement. Through the whole of his course at Harrow, he acquired for himself honourable distinction amongst his schoolfellows, and passed through the different classes, attended, not with the approbation only, but, with the admiration of his tutors; and such was the rapidity of his progress, that in January, 1761, before he had quite completed his fourteenth year, he arrived at the first place in the first form.*

The following interesting account of his childhood (which I am sure the Society will pardon my quoting) was given to Mr. Lynes by Parr's only sister, Mrs. Bowyear. "My brother might be styled slovenly in his dress. I do not recollect that he entered much into the usual sports of boys; he was from childhood of a studious turn of mind, but with me he was playful, though, I must confess, at times rather obstreperous, as he would approach me with clenched fists, though in perfect good humour; indeed he was for many years a kind, affectionate, brother, and anxious for my welfare and happiness. His earliest study and longest cherished delight, next to Mother Goose, was the History of the Seven Champions of Christendom. From the age of nine he evinced a strong inclination for the clerical profession, insomuch that he was accustomed, when our cousins from Eton were with us during the vacations (they, together with myself, forming the congregation), to read the church service, after the due tolling of a bell tied to the banisters by those who officiated as clerk; and sometimes he preached, and we youngsters often thought him prolix enough. He made one sermon for Christmas day, when under twelve years of age, which was shown to the vicar of Harrow, who said it was so good and appropriate a composition, that no clergyman need have been ashamed to deliver it. He substituted for a surplice a shirt of my father's, taken from the press. This reaching the ears of Mr. Saunders, the vicar, he had a gown and cassock made for him, with which my brother was highly delighted. So enwrapped was he in his predilection, as even (notwithstanding my father's remonstrances) to persist in reading the burial service over dead birds, kit-

* Field's Life of Parr, vol. i, p. 16.

* Field's Life, vol. i, p. 19.

tens, &c. Another of his amusements was bell-ringing. With a set of his schoolfellows he frequently assembled to ring a peal, and he was proud of being able to raise the tenor, which the joint efforts of two of his companions were unable to effect. He was likewise fond of exhibiting his strength, to the great horror of my father, in the strange exploit of knocking down oxen in the slaughter-house. But he was nevertheless remarkably attached to animals, and seldom from his childhood read in comfort without a pet cat seated on his table. The only battle I recollect hearing of his fighting throughout his schoolboy days, was with Lord Mountstuart, in defence of a worried cat. His attachments of all kinds were very strong. His earliest favourite was his cousin, Tom Parr, who died early, and to him succeeded Frank Parr, Tom's brother, who was captain of Eton school before he was fifteen years of age, and who also died early. They were young brothers of Mr. Parr of Norwich. He always assumed authority among his playmates at home, making his cousins call him uncle. He was, I think, between twelve and thirteen, when, together with Sir William Jones and Dr. Bennet (Bishop of Cloyne), he wrote and acted a play, whether tragedy or comedy I do not recollect."*

It does not appear that Parr when a boy was fond of juvenile pleasures. Dr. Gabriel, upon the authority of Dr. Allen, tells the following anecdote of him when nine years of age. He was sitting on the churchyard gate at Harrow, looking grave and serious, whilst his schoolfellows were playing about. "Sam, why do you not play with the others?" cried Allen. Parr looked at him with seriousness and earnestness, and in a solemn tone replied, "Do you not know, Sir, that I am to be a parson?" His amusements with Bennet and Jones soon after this period were, to act great personages, to harangue and declaim, and to compose plays.†

The first literary attempt of Dr. Parr was reported by himself to have been a drama from the book of Ruth; and he scrupled not to say that, had he been born in Milton's age, he should have been a poet.

In 1761 Parr was removed from Harrow school, his father, as we find in a letter addressed to Dr. Percival, wishing to educate him in his own profession, and for two or three years he attended to his business. As he grew up, however, he felt and avowed the strongest aversion to medical studies; but that aversion was regarded by his parents as little more than youthful folly or caprice. During this period of his life, one or two amusing anecdotes are related of him. Being called from some more agreeable employment to compound medicines in the surgery, he revengefully pointed out to his father a grammatical error he had committed in a Latin prescription, which drew upon him the animated reproof, "Sam, d—— the prescription, make up the mixture." On another occasion, in obedience to orders received, he prepared a prescription which his father had entered, after much hesitation, in the day book, and in which was included a small quantity of laudanum, an article then for the first time cautiously introduced into medical practice. The next day, reporting with some exultation the good effects of his medicine, he expressed, though still hesitatingly, an intention of repeating the dose. "You may do that safely, Sir," said the son. "Don't be rash, boy; beginners are always too bold. How should you know what is safe?" "Because, Sir, when I made up the prescription," replied the son, "I doubled the dose." "Doubled the dose! How dared you do that?" angrily said the father. "Because," coolly rejoined the son, "I saw you hesitate."*

Nothing could overcome young Parr's extreme dislike to the medical profession, and every opportunity he could snatch from the duties imposed upon him by his father he devoted to those literary pursuits in which he most of all delighted, and in which he was most of all qualified to excel. The following anecdote will show the unextinguishable thirst which he possessed for knowledge, and his anxious endeavours to gratify it. After his withdrawal from Harrow school, he yet contrived to secure for himself some of its benefits by the following happy expedient. Being every day informed of the lesson which the head class was to study, he, whilst engaged in the business of the surgery, laid his book containing that lesson open before him, and devoted as much attention to it as he could, in the full expectation of afterwards receiving the aid of Dr. Sumner's comments upon it. For these comments were always at his earnest request conveyed to him by one of his friends, Jones, or Bennet, or Lytton, or some other boy equally smitten with the love of learning.†

About the close of the year 1764, when he had nearly completed his seventeenth year, Parr obtained the permission which he had so long and fervently desired, of exchanging the profession of medicine for that of divinity.‡ With this view, the advantages of academic education and honours

* Johnson's Memoirs, vol. i, p. 16.
† Idem, vol. i, p. 21.

* Field's Life, vol. i, p. 21.
† Field, vol. i, p. 28.
‡ European Magazine, 1809.

became an object most desirable and almost indispensable; but his stepmother was opposed to the expense, and influenced his father to make the condition of his going to the University, his entry as a sizar. This was what his independent spirit could not brook after quitting his schoolfellows as an equal. His father gave him a month to determine whether he would accept the proffered terms, or relinquish college altogether: he chose the latter alternative. It appears, however, that afterwards parental pride was roused, and he obtained the desired permission to proceed to Cambridge, unaccompanied by the humiliating conditions. On his arrival at Emanuel, young Parr confided the small sum of money advanced by his father to the treasurership of his old friend and schoolfellow Dr. Bennet, the late Bishop of Cloyne. His pecuniary necessities, however, soon became pressing, and he determined to leave the university rather than to borrow. On balancing his accounts he found, to his extreme surprise, that he had 3*l.* 17*s.* over and above the full payment of his debts; and such had been the economy of his expenditure, that, he said, had he previously known of any such sum, he should have remained longer.* He was afterwards, through the interest of Dr. Sumner, appointed head assistant of Harrow school, where he was ordained.

I shall not attempt to follow Dr. Parr through all the vicissitudes of his eventful life, his disappointment at not being elected master of Harrow school, and his consequent establishment at Stanmore, with his marriage at the same period, his appointment to the Grammar School at Colchester, and afterwards to that at Norwich. Suffice it to say, that in 1781 he was admitted to the degree of LL.D. by the University of Oxford; and in 1786, Lady Trafford, whose son he had educated, presented him with the perpetual curacy of Hatton, in Warwickshire, worth about 100*l.* per annum, where he resided until his death.

Dr. Parr was particularly partial to athletic sports, although he did not frequently join in them himself. On the high days, specially devoted by the youths of Leam to the noble amusement of a cricket-match, in association with many young men of the surrounding neighbourhood, he was always delighted to appear among them, animating by his presence, by his good humour, and his kind manner, amidst the fragrant fumes of his pipe, the sportive field and sprightly throng, sharing, almost as much as the youngest and the gayest, in all the ardour of the contest, and in the life and joy of the whole scene. The Doctor was also the admirer and advocate of pugilistic encounters among boys. It was amusing to hear him speak of the tacit agreement which subsisted, he said, between himself and his pupils at Stanmore, that all their battles should be fought on a certain spot, of which he commanded a full view from his private room; as thus he could see, without being seen, and enjoy the sport without endangering the loss of his dignity.*

It is well known that Parr was an advocate for most of the ancient rules of scholastic discipline, especially for corporal inflictions. "Lumbos dolare virgis," he considered so essential a process in the business of education, that when asked respecting any one educated by him, "whether he had been his pupil," his usual reply was, "Yes, I flogged him." Introducing one of his pupils to a lady as her guest, he addressed her in the following words:—"Allow me, Madam, to introduce to you an old pupil of mine, whom I have often flogged, and who, I assure you, is all the better for it." †

Dr. Parr used to say, that he once began to write a life of Dr. Johnson, and if he had continued it, it would have been the best thing he had ever written. "I should have related not only everything important about Dr. Johnson, but many things about the men who flourished at the same time;" adding, with an expression of humour, "taking care at the same time to display my own learning." On another occasion, speaking on the same subject—"I once intended to write Johnson's life, and I had read through three shelves of books to prepare myself for it. It would have contained a view of the literature of Europe;" and, making an apology for the proud consciousness which he felt of his own ability, "If I had written it," continued he, "it would have been the third most learned work that has ever yet appeared." To explain himself, he afterwards added, "The most learned work ever published, I consider Bentley on the Epistles of Phalaris;‡ the next, Salmasius on the Hellenistic language." Being reminded of his first interview with Dr. Johnson, he replied, with great vehemence, "*I* remember the interview well; *I* gave him no quarter. The subject of our dispute was the liberty of the press. Dr. Johnson was very great: Whilst he was arguing, I observed that he stamped. Upon this, I stamped. Dr. Johnson said, "Why did you stamp, Dr. Parr?" I replied, "Sir, because *you* stamped: and I was resolved not to give you the

* London Magazine, 1825.

* Field's Life, vol. i, p. 102.
† New Monthly Mag. 1826.
‡ Field's Life, vol. i, p. 165.

advantage *even of a stamp* in the argument." "It is impossible," observes the narrator, "to do justice to his description of the scene; the vehemence, the characteristic pomposity with which it was accompanied, may easily be imagined by those who knew him, but cannot adequately be represented to those who did not."*

The doctor was once asked the following question: As mathematics chiefly are cultivated at Cambridge, and the classics chiefly at Oxford, how comes it that the three greatest classical scholars of the day, Porson, Burney, and himself, were Cambridge men? His answer was this,—"Sir, Cambridge has nothing to do with their learning; they would have been great scholars any-where;" and he has often been heard to say, that "there were three great scholars; of these Porson was the first, Burney the third,—who the second was, it was unnecessary to say."†

Parr's fondness for the pleasures of the table is unquestionable, and he had a few strong antipathies which were carefully registered, and scrupulously remembered by those who invited him to dinner. Amongst these was his aversion to salmon and cheese, neither of which was ever permitted to appear on the tables of those who were acquainted with his peculiarities.

Parr was most vain and jealous of his literary superiority and fame in the manufacture of inscriptions and epitaphs; of these there are upwards of thirty in number; the most celebrated, to the memory of Gibbon, Johnson, Burke, Fox, and Sir John Moore. Dr. Parr and Lord Erskine are said to have been the vainest men of their times. Once, at a dinner, Dr. Parr, in ecstasies with the conversational powers of Lord Erskine, called out to him (though his junior), "My Lord, I mean to write your epitaph." "Dr. Parr," replied the noble lawyer, "it is a temptation to commit suicide!"‡

Many more characteristic points might be adduced of this singularly-gifted man, in conformation of his phrenological development, but I fear that I have already trespassed too long on your valuable time; I cannot, however, refrain from adding the few following observations, by his friend and biographer the Rev. Wm. Field.

"The powers of Parr's mind were of a high order; few surpassed him in quickness of perception, and still fewer have equalled him in the wonderful faculty of a memory, so retentive as to be pronounced almost 'miraculous.'

What he once knew seemed never to be erased from his remembrance; his recollection even of names and dates, and the minute circumstances of facts, rarely failed him. His imagination, vigorous and excursive, was united with a judgment strong and penetrating, though not always sound or correct; and all his intellectual powers were diligently cultivated by deep meditation and constant and careful reading.

"As a scholar, in the opinion of the most competent judges, his learning was vast and various, accurate and profound; he explored the most hidden recesses of ancient erudition; and knew what few even of the learned knew besides himself. Nor should it ever cease to be remembered, that all his literary stores were collected together, not in a state of ease and affluence, but amidst want and privation; not under the warm sunshine of patronage, but beneath the chill shade of obscurity and neglect."

Dr. Parr died on the 6th of March, 1825, in the 78th year of his age. He had long been habituated to look, with the eye of calm anticipation, to the appointed end of all human beings. In his latter years he frequently wrote "directions for his funeral," of which the last bear date March 17, 1824; and these, in the same year, were followed by some "additional directions." In them, he minutely describes the hour and place of interment, the order of the procession, the manner of preparing the church for the occasion, and the mode of conducting the service; he enumerates the clerical friends to be invited, and mentions the persons to be engaged as the bearers of the body; he describes the very ornaments of the coffin, and names the persons to be employed in making it. But the most extraordinary of these directions are the following, "which, however strange," observes his biographer, "they may appear, no doubt originated in the warmth of his affection for his children, and in the sincerity of that respect, with which he ever cherished the memory of his deceased wife.

"I lay particular stress upon the following directions:—My hands must be bound by the crape hatband which I wore at the burial of my daughter Catharine; upon my breast must be placed a piece of flannel, which Catharine wore at her dying moments at Teignmouth; there must be a lock of Madelina's hair enclosed in silk, and wrapped in paper, bearing her name; there must be a lock of Catharine's hair in silk, and paper with her name; there must be a lock of my late wife's hair, preserved in the same way; there must be a lock of Sarah Ryme's hair, preserved in the same way. All these locks of hair must be laid on my bosom, as carefully as possible, covered and fastened with a piece of black silk to keep them together."

* Blackwood's Mag. 1825.
† Ibid.
‡ London Mag., 1825.

* Field, vol. ii, p. 336.

WESTMINSTER MEDICAL SOCIETY.

Saturday, March 17th, 1832.

Dr. STEWART in the Chair.

EPIDEMIC CHOLERA OF LEEDS IN 1825.

THE preliminary business having been disposed of, Mr. Dobson, on the motion of Dr. J. JOHNSON, was requested to give an account of the epidemic diarrhœa occurring in Leeds and its neighbourhood during the summer and autumn of the year 1825, he (Mr. D.) having witnessed the disease, and been concerned in its treatment. Mr. Dobson then presented a paper on the subject, which was read by the Secretary, and of which the following is an abstract:—

Mr. DOBSON prefaced his narrative by remarking that he was induced to submit it to the Society, in consequence of remarks made by Dr. Johnson at the previous meeting, that there were materials at home for studying the disease without going to India, and from the conviction that it might assist in throwing some light on the present momentous question. He had superintended many of the cases which occurred at that period, and his opinion was, that the facts he had to detail would tend to show that the disease now existing in London, and its vicinity was not a new one. He should mention, that the common English cholera always prevailed to a greater or less extent in the neighbourhood to which he referred during every summer. But in the summer of 1825, it presented so many entirely new, anomalous, and appalling features, that the medical men were alarmed at its appearance. The most energetic measures were adopted, not so much to arrest the progress of its ravages, as to relieve the anguish of the sufferers. Prophylactic measures were a secondary consideration. Every day new victims were "immolated, or suffered from its attack." It was not until the middle of the winter that the disease disappeared. Language was inadequate to depict the distress which prevailed. The disease presented itself under various forms, and might be considered under three divisions, viz. Cholera, Diarrhœa, and Dysentery. The following quotation from Mr. Thackrah's pamphlet, would show the character of the season of 1825, the nature of the locality where the disease occurred, and the character of the residents:—

"In May 1825, three-eights more rain fell than the usual quantity, and the wind was E. nearly the whole month; the weather was showery, and on some days particularly sultry; on the 12th the thermometer reached 87°; the wind was variable, but principally W. In July the weather was sultry, the barometer high, the winds variable, and the quantity of rain particularly small. About the 3rd of August, the drought which had prevailed was relieved by refreshing showers: during this month the quantity of rain exceeded the average; the weather was sultry, the barometer low, and the wind generally W. Gawthorpe, a place four miles W. of Wakefield, on high ground, and almost destitute of wood, is inhabited chiefly by weavers, who, though not generally poor, are dirty in their persons, and suffer their houses to become uncommonly filthy. Few if any persons above the lowest class reside in this village. As there is no public well, the water drunk is obtained chiefly from ponds. Of seventy houses which we examined *seriatim*, seventeen had been exempt from cholera and dysentery."

This referred to the month of September. Not one house, when the disease ceased, had escaped. In May and June cholera appeared under its usual form, but in the months of July and August it assumed a more aggravated character, and the result was more fatal. An individual would be suddenly attacked with pain in the abdomen, particularly around the umbilicus; purging almost immediately ensued, first of feculent matter, and then followed watery or mucous stools, with excruciating tormina before each evacuation, and painful tenesmus. If appropriate remedies were promptly applied at this stage, relief was generally obtained. Some cases baffled every endeavour even to mitigate them; others admitted of temporary relief, but soon the disease would re-appear, and with augmented vigour. The most alarming prostration of strength, with vomiting and purging of large quantities of fluid resembling gruel; cramps of the legs, and sometimes of the arms; intense spasmodic contractions of the abdominal muscles; pulse quick, and often imperceptible; coldness and clammy state of the skin, sometimes confined to the extremities, at others extending over the whole surface; countenance vacant, face of a most ghastly aspect; eyes sunk in the sockets, and motionless; the eye-lids, and around them, of a purple, or rather leaden hue; tongue dry and brown; lips purple; the face, even of a young and plump individual, became contracted like that of a toothless old man; the hands presented to the sight and touch the appearance of death; respiration was slow, labouring, and imperfect. In another individual the complaint would suddenly appear in all its malignancy without any premonitory symptoms. He would sink in two or three hours under accumulated agonies, without the slightest re-action taking place. During the autumnal months the disease

still prevailed, but it was now ushered in by prostration of strength, severe diarrhœa, and typhoid fever. Vomiting generally existed for a few days previous to the more urgent symptoms, only when fluid was taken, or, at least, seldom otherwise, and then only of what had been previously swallowed. The intestinal evacuations changed their character from a gruelly fluid to a soft gelatinous mass, or to mucus streaked with blood; sometimes they were like the washings of meat; large flakes of lymph floating in the liquid; frequently scybala of the size of a walnut. Previous to dissolution, discharges of blood took place from the bowels to a considerable amount. The most prominent typhoid symptoms marked the disease throughout. Death, in general, ensued on the fourth or fifth day. The treatment consisted in the application of heat to the surface, and the exhibition of brandy, ammonia, and opium, in doses of two or three grains in a pill. Tincture of opium could seldom be retained on the stomach, nor even brandy in many cases. Fluids constantly provoked vomiting, and were therefore strictly prohibited in every case.

Diarrhœa.—The next point to which Mr. Dobson referred, was the diarrhœa which existed unconnected with cholera. Diarrhœa during this season was very prevalent. It arose frequently without any assignable cause; occasionally it was traceable to indigestible food. The symptoms were, frequent purging and griping pains in the bowels, especially before each evacuation. The characters of the dejected matters were analogous to those observed in the cholera cases. Extreme debility, typhoid pulse, cold extremities, rigors and tenesmus. Relief could sometimes be procured in a few hours, especially if assistance was promptly obtained; but if neglected for a few days, the complaint would resist every means for weeks or even months. Not many deaths occur in this form of the disease. The remedies were simple, and generally efficient; three or four grains of calomel with one or two of opium in a pill were given every four hours, until the spasms and purging ceased, followed by a dose of oleum ricini, or tinct. rhei. If the disease relapsed, recourse was again had to the calomel and opium, conjoined perhaps with an aromatic cretaceous mixture.

Dysentery.—Many of the cases of diarrhœa when continued beyond the fourth or fifth day, especially when medicine had not been taken, resolved themselves into severe cholera or dysentery. When the latter, there was an enormous discharge of blood from the bowels, sometimes pure and uncoagulated, at others mixed with a brown watery fluid. Scybala of a large size were often evacuated. There were most distressing tenesmus and pain at the arch and sigmoid flexure of the colon. Typhoid fever of a most malignant character, delirium, suffused eyes, headach, brown parched tongue, sordes about the teeth, quick pulse, involuntary discharge from the bowels, and retention of urine. Dysentery required the most energetic treatment, six or ten grains of calomel, with two or three of opium, were given every two or three hours, until mercurial action was fully established. When the mouth could be fully affected, recovery soon took place. General bleeding was seldom resorted to; leeches at the seat of pain, and hot fomentations, and a ball of opium placed in the rectum, relieved the tenesmus.

Dr. GREGORY thought that no person could confound such an epidemic as this with the cholera which had recently made its appearance in the metropolis, the former being a dysentery, and totally distinct from the present cholera. He had hitherto abstained from expressing his opinions on this subject in the society, because it was formerly impossible to argue the question. But he now stated, that he believed there was a malignant disease in London, identical with that of Asia, which had never before been seen here. Of this disease it was the *severe* cases that should be studied. Dr. Webster, some time since, read a paper on English cholera, and now wished to prove, that the present cholera was identical with the diarrhœa which he then described, his remedy for which was a little calomel, or if that should fail, the following recipe; 4 drachms of *mist. camph.*, 4 drachms of *aqua linnamonii*, vij grs. of *sulph. magnes.* and vj ℥ *tinct. opii.* A pretty cure for the raging cholera! With regard to its mode of communication, he had seen it attack the husband after attendance on the wife, and the son after attending the father, and so on; and he had come to the conclusion, that it was most certainly contagious—as much so as typhus,—nay, if they pressed him hard, he would say, as contagious as small-pox.

Dr. JAMES JOHNSON said, he had taken the trouble to go through the sixty-five reports of the Madras Board, and the result of his research was, that in not one of them was consecutive fever mentioned, and in only three reports was preliminary diarrhœa adverted to. As to Dr. Gregory's remark about studying severe cases, he would ask the Dr. whether, if he had only seen confluent small-pox, he would be able to recognise the common?

Mr. DAVIES, of Ware, said that he had on the previous evening expressed his satisfaction that the majority of the members of the society were of opinion that no new disease had visited the metropolis, but at

the same time he accompanied this by a doubt as to the correctness of the conclusion. Since that time he had seen the disease, and was convinced that it was distinct from anything he had witnessed before. Mr. Dobson spoke of brown tongue, tenesmus, and dysentery, as characteristics of the epidemic of 1825; but were these symptoms found in the present disease?

Mr. WALKER was desirous of drawing attention to the plan of treatment recommended by Dr. Negri in his letter to Dr. Barry. Mr. Walker had found the bark, as directed by Dr. Negri, to be a very efficient medicine. After attentive inquiry, he had found most of the cholera cases preceded or accompanied by fits of shivering, assuming a regular periodicity, a fact which suggested the use of quinine.

After a few further observations, the Society adjourned. At the next meeting Mr. Hunt will introduce the subject of the treatment of diarrhœa.

LONDON MEDICAL SOCIETY.

Monday, March 19, 1832.

Dr. BURNE in the Chair.

DISCUSSION ON THE CHOLERA.

AT another society, Dr. JOHNSON endeavoured to show, last week, on the authority of a work written by Mr. Thackrah, of Leeds, that an epidemic which prevailed in that town in 1825, presented exactly the same characters as the malignant cholera now existing in London. At the London Medical Society, on Monday last, Mr. WALKER, who was house-surgeon to the Leeds Dispensary at that time, said that he did not consider there was a striking similarity between the diseases of these two occasions. It was true that there were in the Leeds cases, vomiting, cramps, and dysentery, but the medical attendants never thought of the patients dying. There were no such collapses as in the London cases, and Mr. Thackrah himself, when he wrote on the subject of the cases of 1825, in the *Leeds Mercury*, never thought of likening the disease to the Asiatic cholera, with which he believed the disorder at present raging to be identical. A great deal more was said on the subject, but this was the gist of the observations.

In consequence of what passed on this and on a previous occasion elsewhere, Dr. JOHNSON this evening commenced the business of the meeting, by referring to the subject, and a long conversation followed, most of it of no public interest. It must be sufficient for us to say, that Mr. WALKER persisted in his opinion that the two diseases were not the same in character, but that it was the conclusions of Mr. Thackrah, and not any of his statements (for he believed Mr. T. to be a man of unimpeachable veracity) which he disputed in denying the dissimilarity asserted by Mr. Thackrah, to exist. (There was an asperity in the manner of conducting the defence of Mr. Thackrah, which was unpleasant to the members, and as there appeared to be no sufficient cause for acrimonious feelings, the debate was stopped, ostensibly on the ground that it was not orderly to discuss matters which required a reference to statements made at another society.) The proceedings then took a more general turn.

Dr. BLICKE said, there were no symptoms which could be named in the malignant cholera of Asia and England, the like of which he could not show in some other disease. What then did gentlemen mean to point out as distinctive characters of the cholera? Rice-water evacuations were the most frequently quoted, but this he had seen in all other stages of cholera.

Mr. PROCTOR.—Dr. Blicke has seen no disease in this country with the same termination. He has never before seen in England so ugly a customer as the prevailing cholera.

Dr. BLICKE insisted that he had.

The PRESIDENT.—The ordinary cholera in this country seems to me to be the *first stage* of the malignant cholera. Although the majority of cases in this country are ushered in with diarrhœa, yet the attack which distinguishes the cholera is sudden and tremendous vomiting and purging, by which the alimentary canal, if it be not already cleared, is soon freed from all fæcal matter, when the purging of a barley-water fluid with mucous flocculi in it, commences, on the setting in of which, and not until then, the epidemic cholera must be considered as formed. It then proceeds from bad to worse, the blue stage gradually coming on, the pain, vomiting, and cramps ceasing, the patient lying perfectly careless of life, and only wanting to be left to die peaceably. The President then adverted to an account of 94 cases of cholera, published by Dr. Moore (?) in the Calcutta Transactions for 1828, which he had lately read, and expressed his opinion that no one could see one of the cases in London and read that account without acknowledging a striking likeness to exist between them. Of the whole of those cases only one person recovered who had lost the pulse—just as in this country among those in whom it could no longer be felt, recovery was comparatively rare. Farther; one or two who did recover, of the 94 cases, had delirium and signs of consecutive fever, so that in India

the patients had, occasionally, (contrary to the declarations of some) febrile symptoms after the disease.

Dr. Whiting.—I differ from the opinion of Dr. Burne, that the disease is not to be considered as the true epidemic until profuse vomiting and purging have set in. I think that during the early symptoms of the disease, the cases are as much cholera as when there is a total loss of pulse. If indeed the disease is spread by contagion, I am sure that it is as much disseminated by the mild as by the severe cases, and that it is those patients who suffer mildly, who, by going about the town, communicate the disease. As to contagion itself, I have now had such ample opportunities for observing the disease that my mind is quite made up on the subject, and I believe I could bring forward proofs relating to it which would convince the most sceptical.

Dr. Blicke.—Ay, but which way?

Dr. Whiting.—I am ready to enter upon the evidence at any moment it may be desirable to do so.

Other Members.—Dr. Whiting should say to which side his proofs would tend.

The President.—To the side of contagion certainly.*

Dr. Whiting turned to another subject. I have this moment come from a case of cholera of the most horrific kind that I ever witnessed. Indeed I never in my life saw anything equal to it, except in a case of hydrophobia. In fact the case presented so much the appearance of hydrophobia, that I could hardly believe it to be cholera, until I had made inquiries of the friends, when I found there had been vomiting and purging. The patient was racked by the most intense spasms it is possible to conceive, every muscle of the body was in a state of the most plastic contraction, the muscles of the abdomen were gathered up into folds and knots, the knees were drawn up to the head, the arms were forcibly bent, and the whole frame was writhing in the most dreadful agony, the patient shrieking in a manner truly awful. I tried to get down some laudanum, but the attempt was useless. It got to the fauces, but no further. I then tried to administer some by the rectum, but without effect. The spasms and diarrhœa continued until the patient died, and I came hither.

Dr. Burne.—In speaking of *confirmed* cholera, I did not mean that the first stage was not equally cholera, but I had in my eye such cases as I conceived should be reported to a medical board, of which the diarrhœa should rather be considered as the premonitory stage.

The anxiety now to hear something on the subject of contagion (for that branch of the inquiry has for a long time been in abeyance) was revived, and

Mr. Burt again asked Dr. Whiting if he really thought the disease contagious.

Dr. Whiting.—Certainly I do, and am prepared with facts in support of my opinion. It is not easy, however, to lay down in a room in this casual way my reasons for the belief; it requires time and care to present them in a useful form. The subject cannot now be done justice to. A night should either be devoted to it, or it should be let to pass.

Mr. Hooper.—I have not yet quite made up my mind on the subject; instances, however, have occurred to me strongly favouring contagion. A woman was brought into the cholera hospital on Sunday, and her sister, who was in perfect health, came to attend her. The woman died, and the sister went home, but soon after came in herself with the disease, and died in the hospital. The next day also a child in the same family died. Such things as these make me waver, and my impression now is, that when I have seen more of the disease, I shall be a contagionist. I have more than twenty cases of bowel complaints in private families now under treatment, and I believe that if these patients were not in circumstances which enabled them to have plenty of good things and other comforts, and were not treated medicinally, they would all run into a state of collapse. It is the weak who sink most easily. They will sink down with two or three copious motions and die. People in the workhouse—old and weak people, will actually have the disease come on while the surgeon is going round, and die after one or two stools, and I am sure it is the same disease, whether the attack is mild, or proceeds into a state of collapse. To advert to another subject. One of the most important points which can now be discussed by the profession, is the consecutive fever. If you see the patient while the pulse remains, the case may recover, and get into the stage of consecutive fever. From this the patient will get better, but a second time the collapse will come on, and then he will die. This happens to us every now and then. There was one in the hospital last week,—a woman who had so far recovered as to be only waiting for some clothes to get up. On Monday night last I went to the hospital from the Society, and found her with a hard cough which had just come on and soon got worse; the pulse became feeble, the countenance sunk, and she fell back again, and collapse came on a second time, with

* Dr. Whiting had said on the previous Monday evening, "that he had seen enough to convince him that if there was only one contagious disease in this country, that disease was the cholera morbus."

purging and vomiting. A gentleman sat up with her all night, and used the hot-air bath and stimulants freely, and got her out of the state of collapse. However, in the evening, she became bad a third time, and soon after died.

A MEMBER.—Was there any shivering in this case; for it seems to present the character of an intermittent?

Mr. HOOPER.—None at all. I have had many such cases as this, and in none of them shivering.

Mr. SALMON.—The disease seems to be a functional disorder, and not an organic; but of what function? It appears to me to be caused by an arrest of the natural perspiration, which it should therefore be our object to restore, for from all I have read or heard there is no perspiration in the cholera.

Dr. BLICKE.—There is none at all.

Mr. HOOPER.—I never saw a case without it.

Dr. WHITING.—It is abundant.

Mr. SALMON.—Yes; but the moisture you speak of is not the proper secretion of the body.

Dr. WILLIAMS.—What does Mr. Salmon mean by perspiration?

Mr. SALMON.—That which is exuded is not carbonic acid gas, the natural perspiration of the human body. It has been described by all writers abroad that if you can get a profuse and wholesome perspiration you obtain a cure, and the fact that emetics produce an increased action of the skin, and that they will produce benefit in the disease, is a corroboration of my position. Mr. Salmon then instanced certain evidences against contagion which have often before been quoted.

Mr. STEVENS argued that the disease was only an epidemic. He had often known ague begin near water, and after a time appear in parts at a distance from the original source of the disease, in the same manner as in cholera. Yet ague was not allowed to be contagious, though the communication of inhabitants of the first district with those in the second, seemed the only possible source of its existence in the latter,—a thing which was out of the question. There was no difficulty in tracing small-pox.

Mr. KINDON said a few words on the same subject, and observed that he would have every case of cholera reported, mild and severe, to show how comparatively trifling was the mortality which attended them.

Mr. HOLT read the case of a patient who had died under his care. It did not excite discussion.

Dr. JOHNSON, in the course of some observations on the diarrhœa of cholera, recommended that the Indian practice in the first stage of the cholera in Asia, should be adopted in the second stage of the cholera in England; for the two stages were synonymous.

The meeting shortly after adjourned.

There are some things in the above discussion, which, if they do not edify our readers, will at any rate amuse them.

In the course of the discussion on the 12th, Dr. BURNE made some observations, tending to show that the late researches of Delpech were not correctly founded.

Mr. MISKIN mentioned a case attended by severe cramps, for the allaying of which a small quantity of belladonna was successfully employed.

Mr. EVANS, in the course of some observations on the diet in cases of cholera, strongly recommended milk as the best fluid that could be given.

He also related the following facts, as very curious, if not bearing importantly on the question of contagion. An old woman who resided in the house which was turned into the cholera hospital in his district, quitted the place about nine days after its alteration, leaving a little dog behind her, which was at night thrown into the cellar, where the coffins were temporarily deposited, as his pupil wanted to preserve it for the purpose of some anatomical inquiries. It was soon afterwards conveyed to his (Mr. E.'s) house, and fed, and kept well. In a short time, however, the dog fell ill, had purging, vomiting, and most dreadful cramps in the extremities, and speedily died. Two hours after, an examination of the animal was made, when the bowels were found much congested and contracted; there was considerable redness of the small intestines; the gall-bladder was enlarged, and the biliary ducts were contracted; the bladder was rigidly contracted; and, in short, all the more prominent signs which are found in patients who have died of the malignant cholera were present. He would not dwell on the circumstances; but they were curious enough.

COMMUNICATIONS
OF THE
CENTRAL BOARD OF HEALTH
WITH MR. SHELDRAKE.

To the Editor of THE LANCET.

SIR,—I subjoin the copy of a letter which I have just received.

"Central Board of Health, Council Office, Whitehall, 6th Jan. 1832.

"Sir,—In reply to your letter of yesterday's date, I am directed to acquaint you, that as the subject therein alluded to ap-

peared in THE LANCET of Saturday last, the Board are not aware of the necessity of further measures being taken to give it publicity. I am, Sir, your most obedient servant.

W. MACLEAN, Secretary,

"T. Sheldrake, Esq. 73, Upper Berkeley-st., Portman-sq."

This letter confirms a truth which is well known to your readers, viz. that the simple statement of any fact in THE LANCET is of more use to those whom it may concern, than any act of the Central Board of Health. As the particulars of their transaction with me may be useful if made public, I hand them to you, although they will not elevate the parties who constitute that Board in the general esteem. As my letter to Dr. Johnson, published by you on the 31st December, explains itself, I shall say nothing more about it. On December 13th, the immaculate and learned Board published a report, of which I inclosed you a copy, which contains recommendations to charity for the afflicted;—common-place directions as to what might be tried in hopes of stumbling upon something which might by chance be serviceable to the afflicted;—a declaration, that no remedy at all approaching to the nature of a specific has been as yet discovered for this disease; and the equally explicit declaration that, "under such rapidly destructive and almost universal destruction of function, *the most energetic efforts should be directed to reproduce what the disease has* rendered nature unable to keep up, viz. — 1st, *Fluidity, heat, and motion of the blood*; 2nd, *Regulated action in the voluntary and involuntary muscles*; Lastly, *but above every other consideration, renewed energy in the Nervous Centre, the source of all Vitality and Function.*"

This was *virtually* as complete an acknowledgment as could be framed that they were totally ignorant of every thing which could be used with certainty and advantage to cure this disease, which has done so much mischief in the world, and has begun its destructive operations in this country. I, immediately upon seeing this report, wrote to inform its author, that I had for more than forty years employed in my own peculiar practice, with uniform success, a plan of treatment which would, I knew, effect all they had described in the paper I have quoted, as essential to the cure of cholera, and I believed would, in general, effect that cure: that, considering the present state of affairs, I was willing to communicate to them this treatment, unknown as it was to every human being but myself; to make, in their presence, whatever experiments they might choose to have made, to ascertain whether it were or not effectual, and worthy of official publicity; that, if it were found worthy, I would give every information verbally, write any explanation they should think proper, and leave the whole to be made public in whatever way they thought most advantageous. The only stipulation was, that becoming means should be taken to make it properly known that I was the author of the discovery.

After the lapse of three days I received the following under the Great Seal of Office:—

"Central Board of Health, Council Office, Whitehall, 20th Dec. 1831.

"SIR,—In reply to your letter, respecting a cure for certain diseases which you state that you have successfully employed for many years, I am directed to acquaint you, that you are at liberty to make the statement therein alluded to, for the consideration of the Board. I am your most obedient, humble servant,

WM. MACLEAN, Secretary.

"T. Sheldrake, Esq. 73, Upper Berkeley-st., Portman-sq."

Wonderful generosity! A man who has employed fifty years in bringing to its present state of perfection a discovery in physical science, so much out of the ordinary course of such things that many who are told of its effects will not believe the facts to exist till they see them with their own eyes, is allowed to be at liberty to drop the fruits of his labours at their door, that they might afterwards reject them, or garble them, or perhaps convert them to their own profit or credit, while even the reputation of his discovery is transferred from himself to some retainer of their own. This letter explained the whole of their scheme. I saw that I had placed my fore-foot in the mud; but as I knew it would be more discreditable to retreat than to go through it, I determined to proceed with the business, using however due caution, and accordingly I immediately wrote a letter, of which I now give the material part.

"December 21, 1831.

"To the Central Board of Health,—Gentlemen, I beg to say, that if I use the liberty you have given me in any other way than I proposed in my first letter, *it may be injurious to my own reputation.* The proposal which I then made, and am still desirous to act upon, is this:—

"I will attend you at any time you may direct, giving me one clear day to prepare myself. I will then produce the patients I have already mentioned, whose cases are of such a description, that few members of the profession, who have not seen them treated by me, *will believe that they could be cured by any means whatever.*

"I will at the same time submit a written statement of all the means I employed

to effect those cures, and will explain, *verbally*, any particulars which may be in your opinion described in an unsatisfactory manner. This will show the *power* of my treatment when applied to diseases for which it is adapted.

"I will also explain, and, if you please, demonstrate, that the treatment I have invented and used in my own practice during forty years, with uniform success, possesses all those properties described by the Doctors Russell and Barry, in your report of December 18th, as indispensable to any mode of treatment which can afford even a chance of rescuing from destruction patients afflicted with the spasmodic cholera. When these facts and these patients have been laid before you, you will form your own judgment of the value of that treatment which is now offered to the public through your intervention. I am yours, &c.

"T. SHELDRAKE.

"No. 73, Upper Berkeley Street,
Portman Square."

After an interval of several days I received a letter directing me to attend the Board on Saturday, December 31st, at twelve o'clock; I did so. Though there was a full Board, all its members were unknown to me, though I have since learned their names. To the Doctors Barry and Russell I was introduced by name. They were my principal, indeed my only, examiners; all the others remained silent while I was in the room. Much polite conversation took place which need not be recorded, because the real business is comprised in the questions and answers I now write down.

Dr. Barry said to me,—You promised to show us a curious mechanical instrument, by using which we might raise the circulation of the blood in any patient who has the cholera morbus, to any extent we please, and keep it up for any length of time we may think proper. *Have you that instrument here?*

Mr. Sheldrake.—My meaning has been mistaken: what *I did say* was this:—I would produce before you patients whom I have cured of peculiarities which no one has been able to cure but myself. Their diseases are quite distinct from cholera morbus; but I understand from your report what properties are indispensable in any application to afford a chance of curing that disease. I know that by the means I employ in one part of my practice, I produce fluidity, heat, and motion in the blood of my patients, and carry it to any extent and for any time I think necessary. I regulate and continue the action of the muscles, voluntary and involuntary, to any extent and for any duration I know to be necessary. I prevent cramps and spasms, to which my patients are very liable, and always cure them without creating uneasiness, or causing the patients to make the least exertion, or voluntary action of any kind. This is the power you are said to want, and which I offer to place at your disposal for the benefit of the public, in this serious state of alarm and danger. I have in waiting two patients upon whom I can demonstrate the accuracy of my statements.

Dr. Barry.—It was prudent to bring two patients, because, if one *experiment should fail*, the second can be tried. Let them come in.

The young man entered, carrying something enclosed in a napkin.

Dr. Barry.—Oh, I see you do use instruments after all.

Mr. Sheldrake.—No. (*To the patient.*—Take off the napkin.) That is a model of the youth's leg in the diseased state in which it was when he came to me. By comparing it with the leg as it is at present you will see the amount of the benefit he has received from my treatment.

Dr. Barry.—We don't want to know about lame legs, we want to know what will cure the cholera morbus.

Mr. Sheldrake.—True. The same process which I used to cure that leg will, according to your own showing, enable those who adopt it, to cure the disease you mention, if the powers you have called for will effect it.

The demonstration being required, the patient was seated. I took the foot in my hand and gave it the motion I knew would be necessary; but that motion appeared to be so simple that Dr. Barry said, What good will that do?

Mr. Sheldrake.—It will produce great increase of circulation in the blood immediately.

The Dr. took out his watch, placed his finger on the pulse, counted, calculated, and exclaimed with surprise, "This is wonderful! *His pulse has risen to one hundred and four already!*"

The patient was quite at his ease while I continued the treatment with increasing effect. All present were satisfied.

Dr. Barry.—How can this effect be produced more quickly if it be wanted?

Mr. Sheldrake.—In the short time employed, the pulse has been raised to 104, by acting upon one foot; if two feet had been acted upon, the effect would have been double in the same time. If the two feet and the hands had been acted on by different assistants at the same time, the effect would, at the least, be quadrupled; that is, an extent beyond which, I presume, there would never be occasion to go: but should that be advisable, I have topical applications which might be applied to the whole surface of the body for any length of time, so that

any degree of effect might certainly be attained. I mentioned, particularly, that I would undertake to produce as much circulation as could be required in any patient who had cholera, in *half an hour*. Dr. Russell noticed this, and in the hearing of all present told Dr. Barry that I had said so.

Every one was now satisfied. Drs. Barry and Russell asked if I would communicate to them the recipes to make the preparations I had mentioned. My answer was that, whenever I was called upon to produce them *for publication*, I would do so, and put whatever I knew would be useful into writing, in addition. I received most cordial thanks from Drs. Barry and Russell, for what they were pleased to call my liberal conduct. They added, they would deliberate, and write to me on the subject, and I withdrew.

I now believed that everything was fairly meant; but, when many days passed without my hearing from them, I wrote the following note :—

"Jan. 5, 1832.

"No. 73, Upper Berkeley-st., Portman-sq.

"To the Central Board of Health,—Gentlemen, I beg to ask if you have determined what course shall be pursued to make public the subject I laid before you on Saturday last, as I am desirous to proceed as speedily and as effectually as the subject will admit of; you having observed, very justly, that the subject was important, and pressed too much to allow of delay. I am, Gentlemen, your most obedient servant,

"T. Sheldrake."

I sent this by a special messenger, who was directed to ask for an answer. He was asked if he was acquainted with the business my note related to. He said no: and was then told, that an answer would be sent to me in the course of that day. I then received the letter which I have placed at the head of this paper.

In making this statement I have confined myself to a simple narrative.

I am, Sir, yours, &c,

T. Sheldrake,

73, Upper Berkeley-st., Portman-sq.

Jan. 6th, 1832.

LONDON MEDICAL SOCIETY.

To the Editor of The Lancet.

Sir,—I wish you would allow me to assume the office of critic for a few moments in a matter connected with the London Medical Society, which deserves a word or two of remark. I do not wish to take this duty out of your own hands, but some things occur at the meetings of that institution deserving correction, which may either not come under your notice at all, or may escape your comments if they do, and on this account I crave leave to make a few remarks on one of them, for I do not see that any proposition towards a remedy is likely to be submitted in the Society itself.

Since the cholera has been a subject of discussion there, the members have, many of them, laid aside the good manners which used to be observed towards speakers—I allude to the interruption of those who are addressing the meeting. It is true that the audience is occasionally goaded into a breach of politeness by the prosiness of the speakers themselves, and that there is precedent for a show of impatience during long and inappropriate speeches in other important assemblies. But I do insist either that gentlemen who are allowed to commence an address should be permitted to spin out the whole thread of their discourse uninterrupted by noises, or that the Chairman himself should have power to stop the speeches, and save the time of the Society. Of these two the latter is much to be preferred, for no sign of disapprobation beyond that of "moving to order," or a direct censure uttered by a member *on his legs*, ought ever to be indulged in a medical association. I have of late heard even the President himself, a calm, gentlemanly, and highly philosophic man, who very rarely says anything which might not be listened to with satisfaction, if not positive advantage, most unpleasantly interrupted by scrapings of the feet, coughing, and buzzings of conversation, and if these continue to prevail I do not know that in a month any man in the Society can feel sure when he rises that he shall be permitted to continue his observations for more than two or three minutes. As to the President himself, I do say that as the members have elected him to the chair, they ought to hear him, if it were for half an hour at a time, with the utmost quiet. It was by their judgment that he was placed in the office, and it is now the duty of the members to show respect *to their own decision*. It certainly ought not to be a habit of the President to speak often in his place, and when he does speak it ought rather to be at the close than in the middle of a debate, except, of course, in the discharge of his official duties.

The remedy I would propose for the evil is this. There has been sufficient proof given during the present session, that gentlemen do not all of them know when they have said enough. There are, indeed, some sad prosers at the meetings, and occasionally too a stranger will come in, who not knowing what has been said at the meetings during the last month or two, will, big with important matter, as he conceives, go over

some old and most uninteresting ground in a manner which would wear out the patience of Job. Now the intimations of this ought not to be by private whisperings, and looking about, and clearings of the throat, and shuffling of the feet, and a raised brow and vacant look from the chair; but the President, knowing that the speaker is wasting the time of the Society, ought to rise, and with a dignity and courtesy which he is very unfit for the presidency if he know not how to use, bring the party to the useful point at once, by a question or two, and see if he really has got anything at the end of his speech worth hearing or not. The speaker is often like a witness in a box,—he frequently knows not what is of value to the Society, and will perhaps be induced to empty his whole mind before it: but the President is counsel to the Society, and (with a decisive manner widely different, however, from that of a law court) should by a question or two, or by putting the witness in a proper train of speech, bring the matter at once to issue.

Many a one who reads these remarks will call to mind scores of instances in which the suggestion might with the greatest advantage have been acted on. Independent of this, the auditors, knowing that the President has such a mode of proceeding always open to him, and in his remembrance, would be reconciled to giving attention to things which may not seem quite in point, yet which, under the circumstances, it is desirable to tolerate until the speaker is reminded from the chair itself that his matter is superfluous or extraneous. The patience of the members might be trespassed on a little sometimes, but placed on such a footing as this, abuse of it could not occur often, and would be better borne than rudely stopped by the harsh noises of a common debating society. Trusting my remarks will be received in good spirit, and not forgotten, I am, Sir, your obedient servant,

P. T. Criticus.

March 20th, 1832.

ADDRESS

TO THE

MEDICAL PROFESSION, AND TO THE PUBLIC GENERALLY.

[At the request of the Association, conveyed through the honorary secretaries Mr. King and Dr. Morris, the following document is introduced into our pages.—Ed. L.]

The present alarm respecting the disease which is now said to exist in London and its vicinity under the name of cholera, is maintained by some to be unfounded or exaggerated; while by others it is asserted that this alarm is neither without cause, nor disproportionate to the danger. Whence such diversity of opinion may have originated, has not been ascertained; nor has it been discovered whether there is any reasonable cause for the prevalence of opinions so opposed.

This unsettled state of the public mind has been productive of consequences the most pernicious. Our national relations have been injured; our internal prosperity has been shaken. The medical profession have been aspersed by one party, as encouraging panic without cause; and by another, as concealing from the public eye the real nature and the full extent of the disease.

To clear the character of the medical faculty of these aspersions by inquiring into the actual causes of this general alarm, and to reconcile these conflicting opinions, a medical association has been formed, to investigate the nature, modes of propagation, extent and treatment of that disease, and lay candidly before the public the results of its inquiries. As this Association belongs to no exclusive party, it advocates no exclusive views. The practitioners of whom it is composed espouse no theory; they support no faction: their bond of union is the public good: their common object is the cause of truth. Their interests are promoted neither by encouraging excitement, nor by resisting just alarm. If the disease known under the name of cholera exist in London, their object is to ascertain its extent, to elucidate its causes, to explain its management, and to restore the tranquillity of the public, by an honest statement of the truth.

An Association formed with motives so pure, and an object so important, can have no enemies but those of science and of mankind. They, therefore, look to the public for countenance, and to the profession for assistance. They more especially solicit the co-operation of the medical officers of parishes and guardians of the poor. As they intend no opposition, they anticipate no resistance. In the truest spirit of philanthropy have they been organised, and with the most rigid respect to truth and candour will all their labours be conducted. They feel that the principles which they follow can lead them only to truth, and they are persuaded that the plan upon which these principles shall proceed, can scarcely fail to ensure ultimate success.

The proceedings of the Association shall at all times be open to inspection; the register* for the admission of members is

* At the house of Dr. Morris, 17, Southampton Street, Strand.

now prepared for signatures; and while they express the most friendly feeling towards such as may decline to join them, the Association earnestly solicit and confidently hope for the speedy co-operation of all who are zealously attached to the interests of medical science, and wish sincerely to promote the welfare of the human species.

LEONARD STEWART, M.D.,
Chairman.

THE LANCET.

London, Saturday, March 24, 1832.

THERE is no secrecy in the tactics of the hospital surgeons—in the manœuvres of the Bats—to harass, oppress, and degrade those members of the profession, who have not, like themselves, the good fortune to be thrust into important offices, connected with which, collaterally, there are many pecuniary and other advantages. The miseries to the public, and the insults to the profession, resulting from the workings of the Battish system, are really innumerable. It is one unmitigated machinery of corruption. It was projected and fostered, while monopoly flourished without check, control, or exposure, in a brace of hospitals of incurables situated near Westminster Bridge; and as long as the general laws of our country shall remain un-remodelled—as long as the great *sources* of corruption shall remain un-purified, so long will the streamlets which permeate our charitable, medical, and all other institutions, exhibit the foul characteristics of their origin. In the conduct of our great hospitals, that portion of the "management" which is not left to chance is generally entrusted to men who contrive to view their own interests with a tolerably keen eye. As for the medical officers, they discharge their duties or not with the most perfect self-complacency; and while under an accusation of negligence, advanced by students or by patients, they fear no dread of dismissal from an all-important treasurer, or a self-constituted committee. But, with the fullest conviction of the neglect which the patients in our public institutions suffer, and with an equally well-supplied knowledge of the inefficient, not to say barbarous, surgery to which they are subjected, it does seem to us most extraordinary that hospital surgeons should have the effrontery to thrust themselves into the witness-box of our courts of law, to offer their testimony in support of alleged unskilful treatment adopted by general practitioners. "But they are subpœnaed," it may be said, "and *must* attend." Why, truly, where is the counsel, or where the attorney who would call a witness, unless fully assured, from something he had previously heard or seen, that the evidence would be in favour of his client? But whenever did a hospital surgeon testify to the want of skill of a hospital surgeon? Never. O no! The *esprit de corps* which actuates that body is too strong to admit of such an abandonment of *caste*. When Mr. ROLFE brought his action against Mr. STANLEY for the recovery of damages in consequence of that skilful surgeon having mistaken a piece of flint—lodged one inch from the patella—for a portion of his unfractured and entire patella, up started in the witness-box Sir ASTLEY COOPER, Mr. BRODIE, Mr. GREEN, Mr. TRAVERS, and Mr. ABERNETHY—up they all started into the witness-box, anxious and ready to prove, not only that Mr. STANLEY was a surgeon of first-rate eminence, but that he had treated Mr. ROLFE'S case with consummate skill; in fact, that there had been no neglect, no want of scientific discernment, although Mr. STANLEY persisted, for weeks together, in forcing towards each other a stone and a piece of bone, in order to effect a union between them, the flint at the same time causing a projection of the skin readily recognisable by the eye even of non-professional observers. All the witnesses—the five hospital surgeons above mentioned—were

bold and confident in declaring that no blame was imputable to the defendant, that they themselves would have acted as he had done—thus declaring that they would one and all have mistaken a large piece of flint, situated at one inch from the entire patella, for a portion of that entire patella! Hence, in swearing that Mr. STANLEY had committed no error, had conducted the case skilfully, and that they themselves would have considered and treated it in a similar manner, they swore point blank to the fact, that they would have fallen into that very error which had precipitated Mr. STANLEY into a court of law!

Who can have forgotten the evidence adduced—or rather the oaths taken, by the witnesses for the plaintiff, on the trial of COOPER v. WAKLEY? Up started again, on that occasion, as if by magic, a whole host of hospital surgeons. Men who had never seen the plaintiff operate, swore that he was a skilful surgeon. And others of them, who had not witnessed the fatal affray between him and the unfortunate Stephen Pollard, attested to the care, knowledge, and gentleness, with which that affair was conducted. The hospital surgeons would not admit that there was absence of skill or of care, and said that, all circumstances considered, it was only proper that the patient should continue one hour on the table, enduring the torture inflicted by some ten or twelve instruments. It will be recollected that Mr. ASTON KEY figured on that occasion in the service of a kindred nephew, and that Mr. FREDERICK TYRRELL, —another nephew,—was subpœnaed, but not called. These gentlemen are two of the surgeons of the Borough hospitals. In the late action of Horsenell v. Blicke, these gentlemen appeared in the box as witnesses for the *plaintiff*, whose object it was to prove that he had sustained a grievous injury from the "bad setting" of his leg by the defendant. When did these gentlemen first see the patient? the accident, be it remembered, having occurred in April 1831, and this evidence having been given in February 1832. In reply to a question from Mr. Sergeant WILDE, Mr. KEY said, "The plaintiff was brought to me about *four days* ago, and I examined his leg." And in answer to a question from Mr. POLLOCK, Mr. TYRRELL said, that he had never seen the plaintiff till either three or four days ago. Subsequently to this, Mr. KEY was asked the following question: "Would not a surgeon who took off the splints about six weeks after the bones had been set, be a more competent judge of the manner in which the bones had been set, than any person could be by subsequent examination? This was Mr. KEY's reply:—"I should think a man can judge of the condition of the limb now, as well as he could have done six weeks after the setting!" In speaking of the operation of lithotomy for Mr. BRANSBY COOPER, the *hospital* surgeon, Mr. KEY swore, that "no man could possibly judge of the difficulties but the *operator himself*." Again, when asked in *cross-examination*, "If the patient had walked prematurely, do you not think it might have produced a *displacement*?" What was the only reply he could make? "Certainly—a displacement." Yet, though this "displacement" might have occurred *after* the expiration of six weeks from the accident, Mr. KEY, in his examination in chief, thought that he could state now with as much accuracy as at the end of that six weeks, how the bones had been adjusted by the surgeon!

Here we quit this disagreeable, not to say disgusting, subject. Dr. BLICKE has maintained his ground in this transaction in a manner which entitles him to the thanks of the whole profession. Not the slightest blame was imputable to him; but we cannot say as much for the conduct of two hospital surgeons, who, in coming forward to speak to the want of skill in the treatment of a broken limb by a surgeon in general prac-

tice, could so far forget what was due to the public and their profession as not to state to the Judge and the Jury, that under the most *scientific* treatment—that with the utmost possible care and attention—with the nicest adjustment of splints, and the most judicious application of pads and bandages, fractured limbs will occasionally leave the wards of Guy's and St. Thomas's Hospitals, as crooked as the horns of a Somersetshire ram.

THE students of several of the hospitals, and more particularly those belonging to St. Bartholomew's and St. George's, complain, in justly indignant terms, of the manner in which they are filched of their opportunities of acquiring a knowledge of their profession, by the slovenly, irregular, and often secret manner, in which the post-mortem examinations are conducted. This abuse constitutes a flagrant violation of the *purchased* rights of the pupils. It is, in fact, a fraud committed both upon their pockets and their heads; and if it be not speedily corrected, the fault shall not be chargeable to a want of exertion on the part of THE LANCET.

M. DELPECH ON THE PATHOLOGY OF CHOLERA.

A MOMENTARY attention has been excited by a paper recently published by Professor DELPECH, of Montpellier, in the *Cholera Gazette*, on the anatomical pathology of the malignant cholera. The doctrines of the Professor are exceedingly attractive, and are founded on the results of some dissections he has lately performed at Musselburgh and Glasgow. M. DELPECH alleges that in all these examinations he found considerable infiltration, injection, vascularity, and increase of size, in the leading plexuses and ganglia of the sympathetic system, especially in the solar plexus and semilunar ganglia. On these observations the Professor founds an ingenious tissue of reasoning, explanatory of the symptoms and effects of this dreadful disorder, and confirmatory of the supposed efficacy of bleeding in its treatment.

It was our intention to have laid before our readers a lengthened analysis of the paper in question, when we learned the particulars of some important dissections performed in Newington for the express purpose of testing the accuracy of M. DELPECH's assertions. The examinations alluded to took place on the 5th inst., and were conducted by MM. DELPECH and LŒWENHAGEN. There were also present Sir W. RUSSEL, Mr. HARRISON, the professor of anatomy in the Dublin College of Surgeons, Dr. GREEN, several of the inspectors attached to the Central Board of Health, the parish surgeons of Newington, and other professional gentlemen. The results of the examinations are important, inasmuch as they at least disprove the universality of the facts which M. DELPECH has asserted. Two bodies were opened,—the first that of a woman who died of cholera in seven hours, the second of a girl who died of the same disease in twenty-four hours. The results are curious enough, for we are informed by gentlemen who were present, that while M. DELPECH and his assistant pretended to *demonstrate* injection of the neurilema, &c., of the nerves in question, no other gentleman present could perceive the slightest alteration or change of colour. The importance of this negative testimony must be duly appreciated when we remind our readers that Mr. HARRISON was one of the many who thought that no aberration whatever existed from the normal attributes of the parts in question.

A singular mistake is also stated to have been made by the demonstrator in the first dissection. Having cut down to the vicinity of the right semilunar ganglion, he elevated a small round body on the handle of his scalpel, cut into it, found it softened and disorganized, and then triumphantly exclaimed, "Voilà le gangleon semilunaire!" Some doubt of this body being the veritable ganglion having been expressed by Mr. HARRISON, that gentleman, in order to settle the question, proceeded to trace the splanchnic nerve, and found, as he predicted, that the true ganglion was nearly two inches distant from the softened body originally pointed out, and which, on closer examina-

tion, was found to be *a lobule of the pancreas*, which had been separated from its connexions by the preliminary dissection.

In addition to these facts, we have since been made acquainted with the particulars of a dissection most carefully and slowly performed by Drs. GREEN and O'SHAUGHNESSY, for the purpose of deciding the point at issue. Not the slightest trace of inflammation, injection, morbid vascularity, or softening, could be perceived in any department of the sympathetic system. Dr. GREEN has made preparations of the solar plexus and semilunar ganglia, which afford an indisputable record of the rationality of the scepticism evinced by the bystanders on the first occasion. A letter from Dr. GREEN, detailing the principal facts which we have mentioned, and drawn up at our suggestion, for the satisfaction of the profession, will be found at page 884 of our present number.

Although the universality of M. DELPECH's proposition is thus completely overturned, we still feel too high a respect for his anatomical acquirements, to suppose that he could have been generally mistaken in his dissections in Scotland; on the contrary, we are inclined to believe, that in some cases the alteration he describes may have actually existed.

We were astonished on a perusal of M. DELPECH's paper, to find that in his reasoning on the physiological influence, from his observations, he made not the least allusion to the writings of Mr. HAMILTON BELL on the same subject. In every respect Mr. BELL has long since anticipated M. DELPECH's deductions.

Atlas of Surgical Apparatus; being a series of delineations of the most important Mechanical Auxiliaries of Surgery, with descriptive letter-press. By H. T. CHAPMAN. London: Highley. 1832. 4to. Also,—

A Brief Description of Surgical Apparatus; intended to accompany the Series of Delineations. By H. T. CHAPMAN. 8vo.

WHATEVER advantages may result in practice from a deep and scientific knowledge of our profession, it will not, we believe, be denied that hundreds, if not thousands, of surgeons, have forfeited the confidence of their patients from a want of neatness, tact, and mechanical ingenuity, in relation to what are denominated the "minor operations" of surgery. Who so blind as not to perceive whether venesection be skilfully and successfully executed—whether bandages be neatly applied—or whether splints and plasters be aptly and nicely adjusted? Nor should it be supposed that dexterity in these mechanical movements is to be acquired in one day's experience; for with weeks, and even years, of practice, there are *Hospital* surgeons whom we could name, who cannot perform the operation of phlebotomy without bungling, nor apply a bandage or splint without raising a smile upon the countenance of the merest tyro. Evidence of defective skill in any one of these trifling instances is sufficient to convince many beholders that the surgeon wants skill and knowledge in every branch of his profession. They fail to admit of such liberal apologies as the one conceded to Mr. TRAVERS by his Hibernian patient in St. Thomas's Hospital, who exclaimed, at the conclusion of rather a tedious amputation of the leg, "By Ja**s, sir, I fear your tools are blunt." The public generally think not of blunt tools, while reflecting upon the failures of an operator, but of blunt perception, and a dull understanding. Fully persuaded of the great, the vast advantages, which must ever result to the surgeon from his possessing an intimate acquaintance with the mode in which the "minor operations" of surgery should be performed, and from commanding that peculiar *tact*—derivable from practice—by which alone they can be executed, we hail the appearance of Mr. Chapman's *Atlas of Surgical Apparatus* with feelings of the most cordial satisfaction. The design of the work is admirable, and its execution almost complete. In addition to home sources of information, Mr. Chapman has prudently drawn from the works of Heister, Brambilla, Richter, and Ott, the Dictionnaire de Chirurgie, Gerdy Traité et Atlas des Bandages, and Bourgery Traité de Petite Chirurgie.

"In our own language," says Mr. Chapman, "although bandages occupy their station in general treatises on Surgery, and in many instances explanatory sketches are added, without which the clearest description will scarcely prove intelligible; the majority of

these works are of some standing, and necessarily defective in modern improvements, omitting a number of extremely useful recent inventions. In short, we cannot be said to possess a collective detail of the surgical mechanism in use at the present day; and this production is an attempt, in some measure, to supply the deficiency."

That the attempt is a successful one admits not of doubt, and the work which Mr. Chapman has here produced may be consulted with advantage by both practitioners and students. It is a disgrace to our hospital surgeons, and a disgrace to our lecturers, that the manipulating department of the science of surgery has been so grossly neglected by them. It is not sufficient that the student *see* the application of bandages and splints—*see* the minor operations of surgery performed—he ought to be instructed, under the guidance of skilful teachers, how to employ his *hands* in the instrumental department of surgery, and, destitute of practical experience in this art, he can never have conferred upon him any reputation for skill or tact in the treatment of strictly surgical cases, unless he possess by nature such a clearness of judgment, acuteness of perception, and nicety of touch, as falls to the lot of few human beings.

In a second edition—and such a useful work is destined to see many—we hope Mr. Chapman will perceive the propriety of supplying a few omissions which are obvious in the present publications—omissions which certainly ought not to exist in an "*Atlas* of Surgical Apparatus." We may instance the absence of illustrations of the instruments employed in obstetric medicine, and in lithotrity. In the notice of the fracture-bed, it is also but too evident that our author has not inspected, with any degree of care, the one invented by Mr. Jerrard, of Honiton—a contrivance not superior, he thinks, to that invented by Mr. Harold, and adopted by Mr. Earle. In deciding the question of the relative merits of the two machines, it is only necessary to state that Mr. Jerrard's bed admits of a steady and well-regulated motion from side to side, whilst not one of the fracture-beds invented by other persons commands any such means of affording ease to the patient, or security from the danger consequent on long-continued pressure upon the soft parts of the body.

Jones's Portable Hot Air Baths.

Having received one of these machines for review, we have submitted it to a trial, and can speak of its efficacy in terms of decided approval. In operation it is highly efficient: but its three chief advantages are these:—First, the extreme facility with which it may be employed; secondly, the vast quantity of hot air which it will generate almost on the instant; and thirdly, the ease with which the heat may be maintained for any length of time. Considering that it will be extensively and advantageously employed, we would suggest to the manufacturer the propriety of adding *handles*, made of some non-conducting material, to the cylinder, or vessel containing the lamps; and with a view to guard against the possibility of accident, we would advise him to introduce into his "directions" a recommendation that that portion of the tube which is placed under the bed-clothes, should be invariably covered by two or three turns of flannel. This last precaution will appear to be indispensable when it is borne in mind, that in spasms of the bowels and cramps of the limbs, the feet and legs cease to be under the control of the will, and are suddenly thrown to all parts of the bed.

Nearly the same observations will apply to *Rippon's Portable Hot Air Bath*. Jones's is the most complete in workmanship and materials, but, consequently, by far the more expensive. The principle of each is much alike.

ST. GEORGE'S HOSPITAL.

CLINICAL REMARKS

MADE BY

Mr. BRODIE.

Monday, March 12, 1832.

ABSCESS IN THE SPHINCTER ANI.

"What prevents an abscess in the sphincter ani muscle, or a fistula in ano, from healing?" asked Mr. Brodie of the *petit* Dr. Hamson, in his journey round the wards on the 12th inst.

The doctor stared and turned paler than usual, but remained silent.

"Why, the constant action of the muscle to be sure," replied Mr. Brodie, kindly helping him out of the hole; "and if you divide it, the abscess will heal, but not otherwise.

ANEURISM OF THE AORTA.

"Did you ever hear of a case of aneurism of the aorta cured?" demanded Mr. Brodie of Peregrine Fernandez, jun., Esq., the house-surgeon.

"I never did, but Dr. Hamson has," replied Peregrine.

The little man again stepped forward and stated, that in Paris he had seen a case of aneurism of the aorta in which the patient's health improved so much whilst he was in the hospital, that he left it and remained absent for *two years*.

"You will find many cases, Gentlemen," continued Mr. Brodie, "in which aneurism of the aorta is said to have been cured (as far as the cure goes) related in Mr. Hodgson's work on the arteries, to which I would more particularly refer you. In cases of aneurism of the aorta below the arch, the artery becomes obliterated. I very much doubt myself the efficacy of bleeding in these cases. It lessens the quantity of blood passing through the artery, to be sure, but it also renders the blood thinner in consistence, and deprives it of that tendency to coagulate which is so highly serviceable to the patient in the natural cure of the disease, by filling up the aneurismal sac, and it also renders the action of the heart very irritable when the bloodletting is over.

DISEASE OF THE BONES OF THE NOSE.

In this case the upper portion of the left ala nasi, near the inner canthus of the eye, is affected. Mr. Brodie at first suspected the case to be syphilitic. The woman had been very much exposed to cold some time back, and she had great soreness of the roof of the mouth, but no affection whatever of the throat. She was admitted into the hospital and took sarsaparilla, under which treatment she grew fat, and gained strength and appetite considerably, but that was all. She left the house and was readmitted, the soreness having left the roof of the mouth and attacked the bones of the nose; portions of the bone are now exfoliating; a most offensive discharge is taking place, which is perceptible to the patient; the left eye is closed, and the eyelid of the same side is inflamed. She has never had any marks or symptoms of syphilis about her,—no nodes or pains in the tibia at night,—no pain in the chest. She took some bitter pills some time ago, which were prescribed for her by a private practitioner, but *does not believe that she ever took a grain of mercury in her life*. There is at present no soreness of the throat, and no remains of the original affection of the roof of the mouth. Several of her teeth have fallen out, but there is every reason to believe that the account which she gives of her symptoms is a correct one.

Mr. Brodie remarked, "Well, Gentlemen, here is a most interesting case which presents some formidable symptoms, and which, if they get the better of us, will inevitably lead on to the patient's destruction. If these cases are left alone, and the violent symptoms are not checked, they terminate in affection of the brain, and the patient becomes epileptic or maniacal. The late Mr. Rose had a case of syphilis in which the roof of the mouth was affected and then the bones of the nose also. At that time it was not the fashion to give mercury in syphilis, and he (Mr. Rose) did not give it. He took me one day to see the patient when the bones of the nose were affected, and I recommended mercury, but it was too late, the patient became maniacal, and I believe shortly afterwards died. There was an officer in Greece who had a somewhat similar affection, which was treated with some simple remedies in that country which did him no good. He came home to London by way of Paris, and whilst at Paris he met with Dr. Wilson of this hospital, who advised him to see, and, I believe, took him to, Dupuytren, who ordered him to rub in; he was seen by nearly all the surgeons in Paris, French and English, and they were all astonished at Dupuytren's method of treatment, declaring that it would prove the death of the gentleman. He however persisted in rubbing in, and was ultimately cured. A lady who was attended by Dr. Blundell, had some affection of the bones. Mr. Blizard was called in, and he said, "Oh! I shall make her rub in ʒss every day." Dr. Blundell was frightened; the lady however followed up the rubbing-in, and was cured. Sir Everard Home had a patient who had pains in the bones, &c., but had never had a symptom of syphilis,—I believe only a slight gonorrhœa twenty years before. Sir Everard, however, took it into his head that he had nevertheless a syphilitic taint in his constitution. This happened in the year 1810, when I was quite a boy, and I recollect being very much astonished when I heard of Sir Everard's mode of treatment. Whether good or bad, however, the patient recovered under it. The late Dr. Pearson, when he was physician to the Lock Hospital, never would give mercury in these cases, but Mr. Blizard was uncommonly successful in treating his patients with mercury. In these cases, however, where mercury has once been given, I am not so bold as to repeat it; but in this case where no mercury has been given, we shall see what effect it will have. Let ʒi of ung. hydrarg. tart. be rubbed in every night, and the ulcer be dressed with the black-wash."

Dr. Seymour commenced a course of clinical lectures at this hospital on Tuesday last, March 20, at half-past three P.M., and will continue them every week during this and the two succeeding months. These lectures are open to all the pupils of the hospital.